Income Maintenance, Work Effort, and the Canadian Mincome Experiment

Derek Hum
Wayne Simpson

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Acknowledgments

If research studies, like good wine, improve with age, then this study should have considerable appeal to researchers interested in labour supply and income maintenance policy. The origins of this study can be traced to 1975 when Derek Hum, on leave from the University of Manitoba, joined the Manitoba Basic Annual Income Experiment (Mincome) as research director. It is well known that Mincome did not see to completion the original research program, especially the anticipated study of work incentive questions. Mincome officially ended in 1979, with much of the data stored in archives and in a state not easily accessible to the academic community. However, interest in the experimental results and income maintenance and other social policy issues survived the abrupt end of the project. Whenever discussion of welfare reform occurred, or mention was made of the possibility of a guaranteed income program in Canada, Mincome and its unexamined data would be recalled. Thus when some of the data from the experiment became available through the Institute for Social and Economic Research (ISER) at the University of Manitoba in 1983, we began to formulate a research plan. This plan was much less ambitious than the original and involved us in our roles as independent academics.

Why did this study take so long? Lack of funds at every stage of the research is certainly the main factor. When other income maintenance experiments in the United States were funding multimillion dollar research programs, Canadian research funding on this issue, as on so many others, paled in comparison. That research on labour supply was ever completed at all owes a debt to many sources. Health and Welfare Canada provided funds to the ISER to document the data-capture procedures and to prepare rudimentary electronically readable files. The Social Sciences and Humanities Research Council of Canada (SSHRC) awarded us a substantial research grant over three years in order to prepare, edit, and conduct a preliminary analysis of the data. Also during this time, Labour Canada supported our work in the general area of labour supply and income maintenance matters. Finally, we owe a tremendous debt to the Economic Council of Canada which supported the writing of this study.

We also owe a debt to a great many individuals. Don Sabourin, statistician and programmer with Mincome, brought his experience to the ISER and to our research team. Greg Mason directed the ISER and supervised the preparation of the Mincome data, no small task. Robert Amano, Gobind Ganga, and Carolyn Hill provided valuable research assistance at various times between 1986 and 1989. Alexander Basilevsky, formerly senior statistician with Mincome, provided invaluable advice. Throughout the years, our colleagues in government and at universities throughout Canada have offered encouragement as well as helpful comments at conferences, seminars, and workshops. The list is extremely long, but we would especially single out Gordon Betcherman, Martin Browning, Norman Cameron, Eden Cloutier, Martin Dooley, Surendra Gera, Lars Osberg, and Les Robb.

A special acknowledgment and thanks must be given to Brian Powell, Director of Social Policy at the Economic Council of Canada. Originally associated with the Seattle-Denver income maintenance experiment, he was one of the original authors of Mincome's research design and he also directed the federal Experimental Research Unit at Health and Welfare Canada. His knowledge and experience of Mincome therefore brought additional guidance to this study; he recognized its potential and strongly supported its completion.

We would also like to thank the SSHRC and St. John's College at the University of Manitoba for research leave fellowships to Wayne Simpson in 1985-86 and the Economic Council of Canada for a visiting research fellowship in Ottawa to Derek Hum in 1989. And we must acknowledge the patience of our families, who must at times tire of hearing about labour supply, income maintenance, and Mincome. Finally, after so many years of discussion and joint effort, it is impossible to tell where one individual's contribution ends and another's begins. Errors are therefore cost-shared on a fifty-fifty basis. And while each author often took responsibility for different aspects of the study or for the first draft of various sections, the final result is the product of an equal collaboration. Authorship order is alphabetical.

Foreword

Together, personal income taxes and transfers play a fundamental role in the redistribution of incomes of Canadians. In assessing the extent to which governments effect this redistribution, one often refers to a tax-transfer system that balances the provision of financial assistance to those in need with the collection of taxes from those able to pay. In fact, the system is comprised of a number of diverse programs, many of which are complex in their own right. Over the years, but particularly during the 1960s and 1970s, considerable effort has been spent in the examination of this system and its component programs, with a view to identifying potential improvements in the areas of fairness, effectiveness, efficiency, and simplicity. These interests led to one of the most significant social experiments in Canada's history, namely the Manitoba Basic Annual Income Experiment (Mincome).

Central to much of the research on the tax-transfer system has been the question of the impact of income transfer programs on the supply of labour. There are two key elements to the reliability of research in this area, namely sound methodologies and good data. Efforts to enhance the econometric analysis of labour supply behaviour over the last 15 years, spurred on largely by the availability of longitudinal data from income transfer experiments in the United States, have produced substantial results. While benefiting from these improved methodologies, the estimates of Canadian labour supply have typically been restricted to those derived from cross-sectional data. The availability of longitudinal data from the Mincome experiment now provides the opportunity to examine the work effort in Canada in a more rigorous fashion.

As part of its work in the area of social policy, the Council is pleased to have supported this study by Professors Derek Hum and Wayne Simpson of the University of Manitoba. They not only provide a useful summary and review of the various estimates of labour supply behaviour derived over several years, but also report in detail on the results obtained using the longitudinal data derived from the Mincome experiment. This research will undoubtedly hold special interest for Canadian economists and policymakers concerned with the trade-offs between equity and efficiency as they relate to labour markets.

Judith Maxwell
Chairman

Summary and Conclusions

Scope and Outline of the Study

Poverty in Canada is an enduring social problem. Public interest in welfare reform peaked in the 1960s, leading to extensive research and five large-scale social experiments designed to assess the consequences of a guaranteed annual income or a negative income tax program. Of special importance was the issue of individual work incentives under such a program. But despite a clear theoretical understanding of the problem, wide availability of numerous data sets of high quality, and impressive econometric analyses of the question, the effects of alternative income maintenance programs on labour supply remain controversial. This study addresses this controversy in two ways: first, we survey labour supply research to date in the United States and Canada (Part A) and, second, we examine labour supply behaviour in the Canadian experiment (Part B). Part A reviews the empirical research on labour supply, paying close attention to those aspects that affect income maintenance programs. The survey considers both the non-experimental evidence and the experimental evidence from the United States and concentrates on the possible reasons why research results differ, such as the data source, the time period of analysis, the estimation technique, and whether or not the data were produced by experimental trial programs. Part B is an investigation of the data produced from Mincome, the Canadian experimental test of guaranteed income. This research should hold special interest for Canadian policymakers since Mincome represents the only available data concerning the work response of Canadians to a guaranteed income.

The contributions of the study include an update of earlier surveys of U.S. research, a survey of Canadian research on the issue, specific emphasis on family labour supply, and an assessment of the Canadian experimental evidence in the context of earlier experimental and non-experimental research.

Poverty in Canada and Public Policy

The Economic Council of Canada reported in 1968 that poverty among Canadians was widespread beyond belief. The government quickly declared war on poverty, and there was talk of a guaranteed annual income for all Canadians. In the 1970s, Canada conducted an experimental trial of the guaranteed income idea, based upon the design of a negative income tax. The purpose of the test was to address worries about the idea's feasibility, program costs, and, most controversial of all, the extent to which a guaranteed income might lead Canadians to work less.

The 1980s brought yet new developments. Long-standing hallmarks of Canadian social programs such as the universality principle were openly challenged. The mood to reform emerged once more though circumstances had changed. Less than two decades after the disquieting figures from the Economic Council of Canada telling Canadians about the extent of poverty, the Royal Commission on the Economic Union and Development Prospects for Canada (the Macdonald Commission) advocated a major revamping of our social security system. It proposed that Canada implement a Universal Income Security Program that, in essence, amounted to a guaranteed annual income. The outrageous idea in the 1960s that government should provide a guaranteed income to everyone now became a centrepiece recommendation of a royal commission.

The Negative Income Tax Idea

Proposals to alleviate poverty through government guarantee of a minimum income are straightforward enough. Three factors influenced the design proposals of a negative income tax, namely, the view that poverty can be defined in terms of income; the fact that the income tax system is an appropriate instrument for welfare reform; and the fascination (or infatuation) of economists with the logical symmetry of the income tax system. But central to both the political acceptability and economic feasibility of the negative income tax was the controversy concerning the labour supply effects. Conventional economic theory suggests that giving able-bodied persons a guaranteed annual income with no strings attached might cause them to work less. Unfortunately, reliable estimates of work reduction due to a guaranteed annual income were not available and, in the opinion of many researchers, were unlikely forthcoming from conventional data analysis. The idea of an experimental test of the guaranteed annual income appeared the next logical step.

Social Experiments in Guaranteed Income Plans

The first in a series of large-scale social experiments in North America – the New Jersey Graduated Work Incentive Experiment – began enrolling families in a guaranteed income program in the summer of 1968, about the time the Economic Council of Canada released its Fifth Annual Review outlining the extent of poverty in Canada. Shortly after, Canada embarked on an assessment of its social security network, including its own experimental test of guaranteed income in Manitoba. The Manitoba Basic Annual Income Experiment (dubbed Mincome) cost more than $17 million and made payments to over 1,000 Manitoban families. Experimental sites were selected and payments to families commenced in early 1975.

The Controversy over Work Incentive

Virtually all the early studies of labour supply used U.S. data. Three salient features characterize these U.S. studies: their exclusive use of non-experimental cross-sectional microdata; their exclusive reliance on ordinary-least-square regression techniques; and their focus on the labour supply behaviour of individuals regardless of family status. Taken together, the results suggest that female labour supply is much more responsive to a change in wage rate than is male labour supply. However, the responses of males and females to income changes are more uniform, although still slightly smaller for females than males. Economists calibrate the degree of response in terms of a measure called "elasticity." In this parlance, the larger compensated wage elasticities for females, combined with similar income elasticities to those found for males, imply larger uncompensated wage elasticities for females. Thus the female labour supply schedule appears to be positively sloped from the early literature, while the male labour supply schedule appears perfectly inelastic or even backward-bending. In short, in response to changes in their wage rates, it was thought that women would reduce their work effort greatly while men would not at all.

Although the early evidence has been interpreted in this fashion, there are reasons to question these stylized facts. In particular, there is the problem of women who are not working. Most of the early studies ignore non-workers. This is a more prevalent and serious problem for women than for men, particularly married women. There is also research that suggests that many of the early results are especially sensitive to the inclusion or exclusion of non-workers in the sample. Furthermore, the estimates are also known to depend upon factors such as the level of education attained by the individual and the amount of family net worth. These findings inspire little confidence in the early results unless we understand the effects of such control variables.

Several important developments have occurred in the last decade. These developments may be characterized as either econometric refinements or data improvements. The development of better econometric techniques has certainly improved the analysis of labour supply behaviour. We suggest, however, that availability of better data has been equally, if not more, important and that econometric refinements by themselves cannot hope to solve certain problems, despite impressions created by the published technical literature. Emphasis in the published literature on applied econometric techniques should not make us blind to the contribution better data have made towards understanding labour supply behaviour. It is actually the integration of newer econometric procedures with superior data that yields better empirical analysis of labour supply behaviour.

Considerable academic effort and expertise have been devoted to the estimation of labour supply models. From the standpoint of social policy questions as well as other analyses of the labour market effects of changes to the tax-transfer system, the value of this research lies in the provision of precise estimates of the critical labour supply parameters. As a result, the burgeoning research has been reviewed on a regular basis to extract consensus, or simply "best guess," estimates for applied economic and policy research. The limited amount of recent evidence, including the experimental findings, generally supports the early results of very small, almost exactly offsetting, compensated wage and income elasticities for adult males. Moreover, the evidence on the uncompensated cross-wage elasticity suggests that it is negligible. Hence adult male labour supply appears to be unresponsive to changes in household wages and income.

However, there remains a disturbingly large variation in estimates of female labour supply response with the passage of time. Most of the estimates refer only to married women because of the early view that marital status matters more for women than for men, and that it is married women whose behaviour differs most from men. Yet there are very large estimates of the compensated wage and income elasticities for single women as well. The range of estimates is smaller for studies using experimental data, although still somewhat larger than those for males.

A recent study of the labour supply of married women by Mroz [1987] provides a partial reconciliation of the results. He finds that analyses using non-experimental cross-sectional data are rife with specification errors. Correcting these errors, in particular the treatment of work experience, yields much smaller compensated and uncompensated wage elasticities for married women. These estimates are, in fact, similar to results from experimental data. Mroz [pp. 794-95], in reflecting upon his attempt to sort out the various elasticity estimates for married women, concludes as follows:

> The negative income tax experiments provide additional data sources for cross-validating the income and substitution effects found in this study. It has been argued that these experiments do permit one to examine more "exogenous" variations in wages and incomes than can be found in nonexperimental data. Indeed the range of estimates reported by Moffitt and Kehrer [1981] for the estimates of the wage and income effects for married women from the experimental literature suggests smaller responses than one would infer from a casual examination of the estimates. . . . Given the conceptual difficulties of analyzing the nonrandom treatment and control assignments in the experimental data and the small number of studies directly estimating behavioral responses, we are hesitant to use the experimental results as a strong cross-validation of our estimates. However, we do believe that a thorough examination of the experimental data could provide a strong test of the robustness of the results found in this study.

Thus experimental data sets, while no panacea, provide a valuable source for further study of labour supply response to social programs. In this respect, further assessment of experimental data should be of interest to labour supply research. Canadians, in particular, should be interested in the results from the Mincome experiment, especially since we have noted that empirical research on the subject is sparse in Canada.

Mincome Data and Their Potential Lessons

The federal-provincial review of social security was a highly publicized evaluation of Canada's system of social programs, aimed at achieving new federal legislation for cost sharing. However, the review lapsed without the hoped-for revisions for cost-sharing social assistance. In the meantime, the parallel Mincome project also proved disappointing; it produced no final report and no research on the question of work incentives. Indeed, few Canadians are even aware that Mincome took place or that a wealth of data remains largely unexamined.

Mincome's design involved selecting participants randomly and assigning them to different negative income tax programs or, alternatively, to a control group. The participants assigned to the treatment programs were given monthly income payments for three years. The control group was not entitled to a guaranteed income. Families were selected from three sites: the city of Winnipeg, the community of Dauphin, and a number of small rural communities. Since the prime research question was the extent to which a guaranteed income might affect work response, the experiment excluded the aged, the disabled, and the institutionalized from participation. All participants were interviewed three times a year to obtain detailed information on their work effort. Therefore the Mincome experiment provides unique Canadian panel data on labour supply behaviour in which households have been assigned a variety of tax rates and income guarantees.

The results from Mincome may be compared to those from four U.S. experiments. The labour supply response from Mincome is lower than any of the U.S. results for men and married women, although similar to the results obtained from the Gary experiment for single female heads. The Mincome results imply a labour supply response of 0.8 to 1.6 per cent for men compared to a combined response of 5 per cent from the U.S. experiments; a response of 2.4 to 3 per cent for married women compared to 21 per cent in the United

States; and a response of 3.8 to 5.3 per cent for single female heads compared to 13.2 per cent in the United States. In short, much smaller responses.

This comparison can be misleading, however. The combined U.S. results are dominated by the Seattle-Denver experiment, which had the largest sample, the most complex design, and the largest reported experimental effects. The other U.S. experiments found smaller and statistically insignificant effects as we have with Mincome. Indeed, our results are quite similar to the findings from the two urban experiments, New Jersey and Gary, and differ markedly only from the Seattle-Denver results.

Comparison is also complicated by the different combinations of guarantee levels and tax rates offered to participants in each experiment. Reported experimental effects add together the effects from a series of plans that differ in each experiment. In this sense, every experiment is unique and simple comparisons of the experimental effects, or the combined effects as in Robins [1985], must be used cautiously. In particular, it would be unwise to use the overall reported experimental effects in the analysis of current social policy questions, since the policy option under consideration is unlikely to conform to the particular mixture of plans from one, or a combination, of the experiments.

The estimates of the labour supply response from Mincome just mentioned cannot be easily used for general social policy analysis. They cannot even be easily used for assessing a guaranteed income proposal such as the Universal Income Security Program suggested by the Macdonald Commission because the estimates refer to the specific effect of experimental programs whose features are unlikely to correspond to such proposals. The best way to provide results for general social policy analysis, from an economic standpoint, is to use the Mincome data to estimate a specific structural model of labour supply.

Estimates of Structural Labour Supply

Wage and income elasticities derived from experimental data can be used to evaluate social policy alternatives. Thus this study pays particular attention to estimating wage and income elasticities from structural labour supply models.

The estimates of the structural labour supply from the Mincome experiment are, as perhaps expected, somewhat mixed. On the one hand, some estimates of the compensated wage elasticity are negative and statistically significant, in defiance of economic theory. Moreover, different model specifications yield rather different estimates. On the other hand, our estimates are quite valuable since, for various model specifications, the elasticity results are consistently small in contrast to estimates from cross-sectional data (the Mincome baseline). The compensated wage elasticity never exceeds 0.33 for husbands, 0 for wives, and 0.76 for single female heads, while the income elasticity is never below –0.12 for husbands, –0.15 for wives, and –0.16 for single female heads. These results imply a range of uncompensated wage elasticities of –0.12 to 0.33 for husbands, –0.15 to 0 for wives, and –0.16 to 0.76 for single female heads. The results consequently confirm the small elasticity estimates obtained by Mroz for married women in contrast to the much larger estimates obtained in some other studies. They also confirm the small estimates for husbands, primarily obtained from earlier literature.

The Mincome results also show that the estimator used by Keeley et al. [1978] based on the Seattle-Denver experiment leads to larger compensated wage elasticities and smaller income elasticities than other estimators for the Mincome data, at least for husbands. In view of this result and other questions raised about their estimator, we would advise extreme caution in the use of their U.S. results for Canadian policy analysis.

One novel feature of our analysis is the focus on family labour supply and the compensated cross-wage elasticity. This elasticity is generally positive for wives and, in some models, large and statistically significant. The elasticity is smaller and less often significant for husbands. The results suggest that we cannot ignore the cross-wage effect in double-headed families, at least not for wives.

Another important factor, also a novel feature of our analysis, is the effect of children on labour supply. Preschool children increase labour supply for the husband and reduce it for the wife. These results are significant and consistent across all model specifications. Indeed, this is likely the strongest effect in the Mincome data and it certainly deserves further investigation.

Conclusions

Our conclusions are guarded but reassuring. First, we believe there is a hint of an emerging consensus on the likely range of behaviour. Much of this consensus has been achieved only in recent years and after 20 years of intensive research. Second, our research leads us to believe that the labour supply response to changes in the tax-transfer system, such as those involved in income maintenance programs, will be small. Although precise measurement of labour supply response is a very difficult problem, and one that economists and econometricians have not yet mastered, we have narrowed the range of reasonable estimates to those that indicate that individuals and families are likely to be fairly insensitive to changes in the tax-transfer system facing them.

Labour supply response is an important issue in income maintenance and other social policy evaluation since work disincentives are a powerful argument against such program initiatives. Time and again, suggestions to reform either income support or social insurance programs in Canada have pressed against the fear that altering benefits for those capable of work might prove counterproductive. At its core, the worry is that cash transfers would diminish work incentives. Our results suggest that these fears are largely misplaced. Our review of the evidence on labour supply behaviour has convinced us that experimental data have proven a valuable source of information for evaluating income maintenance policy. We reject the view that the experiments in the United States and Canada were not worthwhile. In our view, the experiments generated much more uniform and precise estimates than the non-experimental literature, gave impetus to many analytic advances in labour supply research, and provided administrative guidance for future policy development. We believe that many quarters have seriously undervalued these benefits.

Indeed, it is our view that policy experimentation should be considered for other social policy issues such as employment and training. The experience gained from previous social experimentation, especially in the United States, provides a basis for effective experimentation in other matters of Canadian public policy. Many of these areas, such as labour supply and income maintenance, are littered with analytical complexities that defy confident evaluation with non-experimental data and could benefit decisively from well-conceived social experimentation. While the cost of social experimentation is high, so too are the costs of repeated non-experimental research and ill-advised social policy decisions.

Part A

A Review of the Evidence on Labour Supply for Income Maintenance Policy

1 Poverty, Work Incentives, and Social Experiments

The Economic Council of Canada reported in 1968 that poverty among Canadians was widespread beyond belief. The government quickly declared war on poverty and introduced several programs to help the poor. The notion of providing a guaranteed annual income to all Canadians was popular then, but doubts were raised about this unconventional idea. Viewpoints were understandably divided. In the 1970s, however, Canada conducted an experimental test of the guaranteed income concept in order to discover the consequences of such a radical proposal. The trial program would answer, it was anticipated, questions about the idea's feasibility, the cost of such a program, and, most sensitive of all, the extent to which a guaranteed income might lead Canadians to work less. At the same time, Canada and the provinces undertook a formal review of the nation's entire social security system. Although the aim of the review was to be new arrangements for sharing the costs of social programs, a guaranteed annual income for all Canadians was never far from centre stage in intergovernmental discussions.

The 1980s brought yet new developments. Long-standing hallmarks of Canadian social programs such as the universality principle were openly challenged. The mood to reform emerged once more though circumstances had changed. Less than two decades after the disquieting figures from the Economic Council of Canada telling Canadians about the extent of poverty, the Royal Commission on the Economic Union and Development Prospects for Canada (the Macdonald Commission) advocated a major revamping of our social security system. It proposed that Canada implement a Universal Income Security Program that, in essence, amounted to a guaranteed annual income for all families. The country had come full circle. The outrageous idea in the 1960s that the government should provide a guaranteed annual income to everyone became a centrepiece recommendation of a royal commission.

Proposals to alleviate poverty through a government guarantee of a minimum income are straightforward enough. Friedman [1963] was among the first to suggest a "negative income tax" in the sense that unused tax exemptions and deductions allowable under the personal income tax system actually be paid to individuals by the government. Lampman [1965*a*, 1965*b*] proposed a subsidy to earnings to bring an individual's income up to some predetermined level. Tobin [1965, 1966] advocated income allowances whereby those with no earnings at all would receive a minimum amount. All these proposals would ensure that families having no earnings whatever would receive a minimum income, hence the term "guaranteed annual income." All these proposals were also related to the income tax system; hence a negative income tax can mean any delivery of income supplementation based upon the personal income tax.[1]

It was the combination of three factors that influenced the design and basic language of the proposals for a negative income tax, namely, the view that poverty can be defined in terms of income; the fact that the tax-transfer mechanism is an appropriate instrument for welfare reform; and the fascination (or the infatuation) of economists with the logical symmetry of the income tax system. But central to both the political acceptability and the economic feasibility of the negative income tax was the controversy concerning the labour supply effects.

Almost all guaranteed income proposals involve a basic support level given to families if they have no other income and some rate of benefit reduction or "offset" for each dollar earned. Clearly, the more generous the program, as measured by high support levels and low taxation rates, the larger will be the financial costs of a guaranteed annual income. This happens because non-workers would receive larger amounts, low-income workers would keep a larger proportion of their earnings, and a larger proportion of the population would be recipients since high guarantees and low tax rates have the effect of raising the income level (the so-called break-even point) below which negative income tax payments are made by the government. Consequently, providing income payments to the poor through a guaranteed annual income can be very costly, depending upon the support level and tax rate chosen. At the same time, Canadians expect those able to work to provide some portion of their own support. But a guaranteed income might lessen work incentives. Unfortunately, reliable estimates of work reduction due to a guaranteed annual income were not available and, in the opinion of many researchers, were unlikely forthcoming from conventional data. The idea of an experimental test of the guaranteed annual income was the next logical step.

The idea of a guaranteed income or negative income tax began moving out of academia during the mid-1960s in the

United States. But viewing the central problem of the negative income tax as a work disincentive issue effectively made the question an economic one. In the jargon of economics, the negative income tax was restated as a controversy regarding wage rate (price) and income elasticities pertinent to labour-leisure choice. The discipline possessed a coherent theory and economists readily demonstrated that the necessary evidence could be gained with an experiment.[2] Thus the first in a series of large-scale social experiments – the New Jersey Graduated Work Incentive Experiment – began enrolling families in a guaranteed income program in the summer of 1968, about the time the Economic Council of Canada released its Fifth Annual Review outlining the extent of poverty in Canada. Shortly after, Canada embarked on an assessment of its social security network, including the possibility of a guaranteed income. Policymakers also began investigating the possibilities concerning a guaranteed annual income.

The Throne Speech (January 4, 1973) opening the Twenty-ninth Parliament called for a review of the nation's social security system. On April 18, 1973, Canada released a *Working Paper on Social Security in Canada* (the Orange Paper), thereby launching the Federal-Provincial Social Security Review. Coinciding with that review, Manitoba's Premier, E. R. Schreyer, stated in a speech to the Canadian Institute of Chartered Accountants in September 1973: "The Government of Manitoba is committed to launching a pilot project – strictly on an experimental basis, in designated urban and rural areas – to determine if the concept of a guaranteed annual income can be translated into effective action." Interest was also growing in Ottawa. When the Minister of National Health and Welfare established a fund to cover 75 per cent of the cost of such experiments, Manitoba submitted to Ottawa a proposal for a negative income tax experiment. On June 4, 1974, Canada and Manitoba signed a formal agreement. The Manitoba Basic Annual Income Experiment (dubbed Mincome) was to cost approximately $17 million, involve payments to over 1,000 Manitoban families, and last three years. Experimental sites were selected and payments to families commenced in the first months of 1975.

The Manitoba Basic Annual Income Experiment was in a unique position to examine the extent of work disincentive caused by a guaranteed income but, unfortunately, time was out of joint. The chronology deserves emphasis. The Manitoba proposal for an experiment was submitted to Ottawa in March 1973, two months after the Throne Speech calling for a social security review and one month before the release of the Orange Paper. During the last year of the social security review (May 1975-June 1976), Mincome was still enrolling participants in the experiment and making initial payments. Mincome never did provide timely answers concerning work incentives for the social security review. Yet, time and again, the question of work incentives appears in any discussion of welfare reform, whether it be in the context of unemployment insurance, social assistance, or job-training programs. The controversy surrounding individual work disincentives and income transfers just will not go away.

The purpose of this study is to review and interpret the evidence concerning the effect of income transfers on labour supply in detail. Its importance to policy development in Canada has long been established. Part A of the study surveys the empirical research on the subject, paying close attention to those aspects that affect income maintenance programs. The survey is confined to the non-experimental evidence in the United States and Canada and the experimental results in the United States. It focuses upon the possible reasons why research results differ, such as the data source, the time period of analysis, the estimation technique, and whether or not the data were produced by experimental trial programs. This portion of the study may be read by those wishing an up-to-date review of the empirical literature concerning estimation of labour supply functions, bearing in mind that no attention is paid to the Mincome data and that the literature is assessed from the special perspective of its relevance to income maintenance policy. A noteworthy contribution of the survey is the inclusion of Canadian research, both published and unpublished. Part B of the study is devoted to an investigation of the data produced from Mincome, the Canadian experimental test of guaranteed income. Analyses of these data should be especially interesting to policymakers since they represent the only available findings respecting the work effort response of Canadians to a guaranteed income. Chapter 9 offers some concluding observations concerning possible lessons for income maintenance and social policy development, based upon our study of the literature as well as our own research and experience.

2 The Early Literature

When the concept of a guaranteed annual income was first considered seriously in the 1960s, empirical analysis of labour supply behaviour was still in its infancy. In fact, it was the debate over the reform of income maintenance policy in the United States at that time that kindled interest in empirical research on labour supply. Concern about the work disincentive effects of income transfer programs has continued unabated to this day. This chapter assesses the early empirical literature on labour supply while the next chapter examines recent developments in this area from several perspectives. Chapter 4 will then provide a synopsis of the current state of empirical labour supply research in Canada and the United States.

The enormous volume of literature on labour supply that has appeared in the last 20 years makes any review a formidable task. We are fortunate, however, in having four excellent surveys from which to begin [Keeley, 1981; Killingsworth, 1983; Pencavel, 1986; Killingsworth and Heckman, 1986]. Our approach will be to build upon these surveys in several ways. First, we provide a corresponding review of the Canadian evidence on labour supply. The aforementioned surveys concentrated on evidence from the United States. Second, we pay attention to issues of particular relevance to income maintenance policy. This includes emphasis on family, as well as individual, labour supply response and on the behaviour of the low-income population. Third, we update these surveys with a review of the empirical research conducted over the last four or five years. The focus again is on Canadian results, where we have tried to include available working papers as well as published work.

Basic Theory of Labour Supply

Interpretation of empirical evidence depends upon the analytical framework used. In particular, economists employ well-developed models of labour supply behaviour derived from consumer theory. The fundamental features of labour supply response can be conveyed in a model of a family with two potential adult workers.[1] We begin by describing the static case because most of the empirical literature has used the static model of consumer behaviour. Dynamic models, in particular life-cycle models, are examined briefly in the next chapter.

A two-adult family is confronted with market wage rates, w_1 and w_2, for the adult members and has unearned income, y. The objective is to choose the hours of work, $h_1 \geq 0$ and $h_2 \geq 0$, for the adult members and the consumption level, $g > 0$, so as to maximize a family utility function [Becker, 1974].[2]

$$U(g, h_1, h_2) \qquad U_1 > 0, U_2 < 0, U_3 < 0, \tag{2.1}$$

subject to the budget constraint

$$w_1 h_1 + w_2 h_2 + y = g, \tag{2.2}$$

where the price of consumption goods is set to unity so that w_1 and w_2 are real wage rates.

The solution to this consumer optimization problem is given by consumption demand and labour supply (or leisure demand) equations. The labour supply equations take the general form of:

$$\begin{aligned} h_1 &= f_1(w_1, w_2, y) > 0, \text{ if } s_1 = w_1, \\ &= 0, \text{ if } s_1 > w_1, \\ h_2 &= f_2(w_1, w_2, y) > 0, \text{ if } s_2 = w_2, \\ &= 0, \text{ if } s_2 > w_2, \end{aligned} \tag{2.3}$$

where $s_1 = -(\partial U/\partial h_1)/m$ and $s_2 = -(\partial U/\partial h_2)/m$ are the shadow wage rates, representing the opportunity cost of time spent in the labour market, and $m = \partial U/\partial g$ is the marginal utility of income.

The Slutsky equations decompose the uncompensated wage effects into a compensated wage, or substitution, effect and an income effect. This can be done for a change in one's own wage

$$\frac{\partial h_i}{\partial w_i} = \frac{\partial f_i^s}{\partial w_i} + h_i \frac{\partial f_i}{\partial y} \qquad (i = 1, 2), \tag{2.4}$$

where $\partial f_i^s/\partial w_i$ indicates that consumer income has been adjusted to permit the consumer to achieve the satisfaction level chosen prior to the wage change. It can also be carried out for a change in the wage of the other adult or a cross-wage effect:

$$\frac{\partial h_i}{\partial w_j} = \frac{\partial f_i^s}{\partial w_j} + h_j \frac{\partial f_i}{\partial y} \qquad (i, j = 1, 2; i \neq j). \tag{2.5}$$

We expect from the standard consumer theory that the compensated wage effect will be positive, the income effect will be negative, and that

$$\frac{\partial f_1^s}{\partial w_2} = \frac{\partial f_2^s}{\partial w_1}. \tag{2.6}$$

Equation 2.6 indicates that the compensated cross-wage effects must be equal, but this does not restrict the sign nor the size of the uncompensated cross-wage effects in any way.

Adjustments to this model for families with a single adult head, with or without children, are straightforward. We simply ignore the second adult wage and any cross-wage effects. The extension to families with more than two adults is also simple, albeit mathematically more cumbersome. Thus the two-adult family model represents a useful compromise between theoretical and mathematical simplicity, on the one hand, and general applicability, on the other.

Economists are particularly concerned about the signs and the size of the compensated wage and income effects and their corresponding elasticities; that is, the compensated wage elasticity,

$$\frac{\partial f_i^s}{\partial w_i} \frac{w_i}{h_i}, \tag{2.7}$$

the income elasticity,

$$\frac{\partial f_i}{\partial y} \frac{y}{h_i} = \frac{w_i \, \partial f_i}{\partial y}, \tag{2.8}$$

and the compensated cross-wage elasticity,

$$\frac{\partial f_i^s}{\partial w_j} \frac{w_j}{h_i}. \tag{2.9}$$

The reason for this interest is that the labour supply or work incentive effects of any guaranteed annual income proposal, or most other income maintenance proposals, can be assessed in terms of these three elasticity factors.[3] Furthermore, estimates of elasticity magnitudes are particularly useful because they are independent of the scale of the original analysis, facilitating comparison across studies and policy simulation analysis. Accordingly, we focus attention on the elasticity estimates obtained in various studies by different researchers.

Implementation of the Theory

Estimation of the labour supply model in the previous section provides a good case study of the strengths and limitations of the application of econometrics to solve economic policy questions. Several points are worth noting. First, because the labour supply model is carefully grounded in the consumer theory, it is well understood in terms of standard microeconomic theory notions of substitution and income effects. Second, a clear practical application or policy focus is the assessment of the work incentive effects of income maintenance programs. Third, the rapid development of computer technology in the last 20 years has eliminated most barriers in the computation of econometric results. Finally, there has been a growing array of rich data sets for analysis.

On this last point, one dramatic change has been the appearance of non-experimental microdata suitable for analysis of labour supply behaviour. In the United States microdata sources include the Panel Study of Income Dynamics, the Current Population Survey, the National Longitudinal Survey, census microdata, and the Survey of Economic Opportunity, all of which were available by 1975. In Canada, microdata sources include the Public Use Sample Tapes of the census, the Survey of Consumer Finance, the 1981 Survey of Work History, and the 1986-87 Labour Market Activity Survey. The Public Use Sample Tapes have been available since the 1971 census, but they contain only limited information on labour supply, namely, the number of hours worked during a reference week. The Survey of Consumer Finance has similar limitations. The other data sets are more recent. Indeed, the first longitudinal or panel survey in Canada, the Labour Market Activity Survey, has just been released, while in the United States, longitudinal surveys of a 10- to15-year duration are available for labour supply analysis.

Stafford [1986] has observed that:

> . . . theoretical and empirical research in labour economics has been broadened and accelerated by the advent of large-scale microdata sets. The use of these sources has allowed us to know some things with much greater certainty – as illustrated by the work on labour supply responses to social insurance and income support programs.

There seems little doubt that microdata offer much greater potential for understanding labour supply behaviour. Hence we shall ignore the very early empirical literature that was based on aggregate time-series data.[4]

An early landmark in the empirical analysis of labour supply is the volume edited by Cain and Watts [1973]. This volume is devoted to the estimation of labour supply effects using the then-emerging microdata sets in the United States and the application of these results to the assessment of income maintenance policy options then under discussion. Cain and Watts also provide a summary of the state of the empirical labour supply research with non-experimental microdata on the eve of a decade of social experimentation on this question.

The contributors to Cain and Watts' volume raise a number of serious issues associated with the estimation of linearized versions of the labour supply equation 2.3. First, they identify the problem of unobservable, and hence omitted, variables. These include "preferences" [Greenberg and Kosters], or "ambition," as well as the "nonpecuniary desirability of the job" [Garfinkel]. Greenberg and Kosters and Fleisher et al. address this difficulty by including the stock of wealth as a control variable, although both sets of authors realize that wealth is not exogenous, but rather endogenous, in a properly specified life-cycle model of labour supply.[5] Garfinkel notes that other variables, such as the level of education, create similar problems. Another approach used by several contributors to reduce unobserved person-specific differences (or worker heterogeneity) is to homogenize the sample by omitting certain categories of workers such as, for example, high-income families, families receiving welfare, persons not in the labour force, rural workers, young and/or old workers, Blacks or Whites, or single workers. This approach not only reduces wage and income variation, and hence estimation efficiency, it also complicates comparisons across studies and makes extrapolation of results to the general population for policy simulation highly questionable.

Contributors to Cain and Watts' volume also point out several problems of measurement respecting crucial variables. Labour supply may be well understood theoretically, but actual measurement is controversial. Cain and Watts [1973, p. 352] note that the 1967 Survey of Economic Opportunity "provides an unusual amount of detail and a variety of measures of labour-force activity." The survey data set produced no fewer than 14 different measures of labour supply from contributors to their volume [Table 9-3, pp. 350-51]. Additionally, there are problems with the measurement of "normal" or "permanent" wages and incomes from the snapshot provided by cross-sectional data, and there are many difficulties related to the inclusion of any transfer income, such as welfare payments, which depends upon labour supply.

A third set of issues revolve around sample truncation and selection bias. In particular, researchers focus on the exclusion of non-workers from the sample. This is an important issue because there is no reason to exclude non-workers on theoretical grounds. Equation 2.3 specifically includes those who decide to supply zero hours. Hall includes non-workers in his analysis by using the now well-established technique of imputing the wages of all workers, including the unobserved wages of non-workers, from wage regressions for the sub-sample of workers. This strategy has the added advantage of potentially reducing the bias arising from errors in the measurement of wages.[6] The disadvantage, however, lies in suppressing much of the variation in wage rates that is actually observed. This complication is acknowledged by the practice of imputing all wages, not just those for non-workers, in order to avoid heteroscedasticity arising from the difference in variation between observed wages for workers and imputed wages for non-workers. Another truncation problem arises if individuals are excluded on the basis of some criteria that depend upon labour supply, such as earnings or income. For example, when evaluating antipoverty programs, a study might limit its sample to those families whose income falls below a specified level. A low-income sample that is drawn to investigate the work incentive effects of income maintenance programs, say, will then exclude some normally poor households that are temporarily not poor, as well as include some households that are momentarily, but not normally, poor. The result will be to bias regression coefficients for labour supply towards zero [Cain and Watts, 1973, pp. 340-48; Hausman and Wise, 1976 and 1977].

Finally, the Cain and Watts volume makes an initial assault on the problem of non-linear budget constraints. Individuals are assumed to respond to after-tax wage rates, but non-linear budget constraints are created by the non-linear tax system. Hall attempts to solve this problem by introducing the concept of "virtual income." This is the equivalent unearned income amount associated with the linear budget constraint implied by the marginal tax rate at any point on the worker's budget curve. Under a progressive income tax, a rising marginal tax rate implies a rising virtual income. This linear approximation permits us to represent the budget constraint by a straight-line tangent to the relevant section of the curve, based on its slope (the after-tax wage rate at the prevailing marginal tax rate) and its intercept at zero hours of work (virtual income).

Various solutions to these problems have been devised over the past decade or so, involving a combination of better econometric analysis and better data. To some extent, the problems are interrelated. The question of the measurement of labour supply, for example, is not independent of the problems of sample truncation and selection bias. The early literature often excluded non-workers simply because their wages were unobserved. The labour supply theory suggests, however, that the decisions of non-workers must be included. Furthermore, the econometric theory shows that selection bias arises from their omission [Heckman, 1979]. Hence inclusion of non-workers became an important feature of later studies.

Although many measures of labour supply have been used, the preferred measure would seem to be one that includes both workers and non-workers, yet also conveys differences in the commitment of time among workers. Thus labour force participation, as used in early Canadian studies of census microdata [e.g., Gunderson, 1977], is difficult to interpret as a labour supply measure. In particular, labour supply elasticities cannot be computed. Weeks worked can be used and elasticities can be computed, but no distinction can be drawn between fully employed full-time workers and fully employed part-time workers who work fewer hours. Thus we are left with "hours worked" as the preferred labour supply variable, preferably measured over an extended period so as to capture some notion of "normal hours worked." We concentrate, therefore, on studies using "hours worked" as the labour supply measure and for which elasticity estimates can be compared. We might also note that "hours worked in a reference week," as is available from either the census or the Survey of Consumer Finance, is less useful than "annual hours worked" or "usual hours worked" because it has much more random variation and, possibly, seasonal and cyclical components that are individual- or job-specific.

Cain and Watts [1973, p. 348] note that a proper measure of labour supply might also include both "hours spent looking for work" and "hours spent in school," since these investments in human capital may be interpreted as labour force participation decisions. Most studies have not attempted to include these activities, however, and we shall simply note that "hours worked" – which may be either "hours paid for" or "hours actually worked" – is still an imperfect measure of the theoretical concept of labour supply.

A Summary of the Early Results

Since there are no early studies of labour supply based on Canadian microdata, we rely on the previous U.S. surveys to summarize the early results. Table 2-1 presents the early results for adult men and Table 2-2 presents the early results for adult women. Many, but not all, of these results are provided by Cain and Watts [1973]. The salient features of these results are: their exclusive use of non-experimental cross-sectional microdata; their exclusive reliance on ordinary-least-square (OLS) regression techniques; and their focus on the labour supply behaviour of individuals regardless of family status. Only those studies that provide substitution and income elasticity estimates are included in Tables 2-1 and 2-2.

Taken together, the results imply that female labour supply is more responsive to wage rate changes than male labour supply. The mean compensated wage elasticity for females from Table 2-2 is 0.86 compared to 0.14 for males from Table 2-1. Moreover, the range of elasticity estimates is larger for females, with estimates as high as 2.86 compared to a maximum of 0.86 for males. In both tables there are a few puzzling negative estimates of the compensated wage elasticity, but they are all small. Theoretically, the minimum elasticity estimate should be zero.

Income elasticity estimates are far more uniform, particularly if we exclude Hall's bizarre figure of 2.10 for females. Excluding Hall's results, the mean income elasticity for women in Table 2-2 is –0.22 compared to –0.25 for men in Table 2-1. All income elasticity estimates but Hall's are negative or zero. The range of estimates is slightly smaller for females (–0.58 to 0) than for males (–0.86 to 0).

The larger compensated wage elasticities for females, combined with similar income elasticities to those found for males, imply larger uncompensated wage elasticities for females. The mean uncompensated wage elasticity for females from Table 2-2 is 0.83 (0.49 excluding Hall's results) compared to –0.09 for males in Table 2-1. Thus the female labour supply schedule appears to be positively sloped from the early literature while the male labour supply schedule appears perfectly inelastic or even backward-bending.

The early evidence has been interpreted in this fashion by many reviewers, including Keeley [1981] and Killingsworth [1983]. Yet there are reasons to question these stylized facts. In particular, there is the problem of the treatment of women who are not working. Most of the early studies ignore non-workers. This is a more prevalent and serious problem for women than men, particularly married women. The exception is Hall whose results, especially the large positive income effect, inspire little confidence. Thus the difference in the labour supply results for males and females implied by the early results – and embedded in textbooks and articles as the conventional wisdom – requires much further scrutiny.

There is also concern about the robustness of these early results. DaVanzo et al. [1976, p. 324] use the 1967 Survey

Table 2-1

Estimates of the Labour Supply Elasticity for Men, Based on Early Non-Experimental Microdata from the United States

			Estimation		Elasticities		
	Data	Special characteristics	Wages[1]	Hours	Compensated wage	Income	Uncompensated wage
Study:							
Kosters [1966]	1960 census, 1/1000 sample	Married; 50-64 years	No	OLS	0.04	–0.14	–0.09[2]
Rosen and Welch [1971]	1960 census, 1/1000 sample	Urban workers	No	OLS	0.14	–0.41	–0.27[3]
Parker [1971]	Survey of Economic Opportunity, 1967	Full-year, full-time workers	. . .	OLS – Below poverty – Above poverty	 0.00 –0.05	 –0.04 –0.07	 –0.04[4] –0.12[4]
Ashenfelter and Heckman [1973]	Survey of Economic Opportunity, 1967	Married; 25-64 years; spouses not working	No	OLS	0.12	–0.27	–0.15[4]
Boskin [1973]	Survey of Economic Opportunity, 1967	Whites; married; 20-59 years	OLS	Wages imputed by OLS	0.12	–0.41	–0.29[3]
Fleisher et al. [1973]	National Longitudinal Survey, 1966 and 1967	Urban workers; married; 45-59 years	No	OLS	0.04	–0.23	–0.19[4]
Garfinkel [1973]	Survey of Economic Opportunity, 1967	Able-bodied; married; 25-61 years; in labour force	No	OLS	0.00	0.00	0.00[3]
Greenberg and Kosters [1973]	Survey of Economic Opportunity, 1967	Family heads	No	OLS	0.20 0.065	–0.29 –0.21	–0.09[4] –0.14[3]
Hall [1973]	Survey of Economic Opportunity, 1967	Whites	OLS	Wages imputed by OLS	0.06	–0.38	–0.32[3]
Hill [1973]	Survey of Economic Opportunity, 1967	Poor Whites Non-poor Whites	No No	OLS OLS	0.47 0.52	–0.68 –0.86	–0.21[5] –0.34[5]
Tella et al. [1971]	Survey of Economic Opportunity, 1967	Family heads; 18-64 years; wages under $3	No	OLS	0.17	–0.14	0.03[4]

Table 2-1 (concl'd.)

	Data	Special characteristics	Estimation		Elasticities		
			Wages[1]	Hours	Compensated wage	Income	Uncompensated wage
Kalachek and Raines [1970]	Current Population Survey, 1967	Whites; 24-61 years; income less than $8,500	OLS	Wages imputed by OLS	0.86	–0.32	0.54[4]
Kniesner [1976]	National Longitudinal Survey, 1967	Spouses working	. . .	OLS	–0.16	–0.01	–0.17[3]
Masters and Garfinkel [1977]	Survey of Economic Opportunity, 1967	Married; 25-64 years; worked previous year	. . .	OLS	0.08	–0.04	0.04[5]
DaVanzo et al., [1976]	Survey of Economic Opportunity, 1967	. . .	. . .	OLS; with and without imputed wages and income	–0.14	–0.004	–0.15[2] –0.19 to 0.04[3]

. . . Figures not appropriate or not applicable.

[1] "No" indicates no wage estimation done; observed wage used in sample of workers only. "OLS" indicates wages imputed using ordinary least squares and applied to sample of workers and non-workers.

[2] Estimates from Pencavel [1986], p. 69.

[3] Estimates from Keeley [1981], pp. 95-99.

[4] Estimates from Cain and Watts [1973], pp. 332-35.

[5] Estimates from Killingsworth [1983], pp. 119-21.

Table 2-2

Estimates of the Labour Supply Elasticity for Women, Based on Early Non-Experimental Microdata from the United States

			Estimation		Elasticities		
	Data	Special characteristics	Wages[1]	Hours	Compensated wage	Income	Uncompensated wage
Study:							
Kalachek and Raines [1970]	Current Population Survey, 1966	Whites; married; 21-64 years; with low incomes	OLS	Wages imputed by OLS	0.80	–0.58	0.22[2]
Parker [1971]	Survey of Economic Opportunity, 1967	Full-time workers; with children		OLS			
			. . .	– Below poverty	–0.11	0.00	–0.11[3]
			No	– Above poverty	–0.12	0.00	–0.12[3]
Tella et al. [1971]	Survey of Economic Opportunity, 1967	Married; family heads; 18-64 years; wages under $3	No	OLS	0.30	–0.23	0.07[3]
			No	OLS	0.63	–0.52	0.11[3]
Boskin [1973]	Survey of Economic Opportunity, 1967	Whites; 21-59 years; 12 largest Standard Metropolitan Statistical Areas (SMSAs)	OLS	OLS for participation and hours; imputed wages	0.29	–0.10	0.19[4]
Hall [1973]	Survey of Economic Opportunity, 1967	Whites; 21-59 years; 12 largest SMSAs	OLS	Wages imputed by OLS; includes non-workers	2.50	2.10	4.60[4]
Masters and Garfinkel [1977]	Survey of Economic Opportunity, 1967	Married; 25-54 years; worked previous year	No	OLS	0.49	–0.06	0.43[2]
Cogan [1977]	National Longitudinal Survey, 1967	Married	No	OLS	0.92	–0.04	0.88[2]
Cogan [1978]	National Longitudinal Survey, 1967	. . .	. . .	OLS	2.86	–0.02	2.84[2]
Rosen [1978]	National Longitudinal Survey, 1967	Blacks and Whites; married; 30-44 years; both spouses working	No	– OLS, Stone-Geary utility function	1.53	–0.47	1.06[4]
			No	– OLS, CES utility function	0.26	–0.42	–0.16[4]

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1 "No" indicates no wage estimation done; observed wage used in sample of workers only. "OLS" indicates wages imputed using ordinary least squares and applied to sample of workers and non-workers.
2 Estimates from Keeley [1981], p. 102.
3 Estimates from Cain and Watts [1973], pp. 336-37.
4 Estimates from Killingsworth [1983], pp. 12-13.

Table 2-3

Estimates of the Cross-Wage Elasticity for Family Labour Supply, Based on Early Non-Experimental Microdata from the United States[1]

	Elasticity with respect to spouse's wage			
	Husbands		Wives	
	Compensated wage	Uncompensated wage	Compensated wage	Uncompensated wage
Study:				
Heckman [1971]	...	0.013[2]	...	–0.083[2]
Wales and Woodland [1976]	...	0.22[2,3]	...	–0.02[2,3]
Wales and Woodland [1977]				
With children	...	0.06[2,3]	...	–0.14[2,3]
Without children	...	0.20[3]	...	0.26[3]
Rosen [1978]				
Stone-Geary utility function	–0.39	–0.83[3]	–0.89	–1.96[3]
CES utility function	–0.06	–0.30[3]	–0.14	–1.11[3]

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1 Cross-section data on working couples.
2 Estimates from McElroy [1981], p. 58.
3 Estimates from Killingsworth [1983], p. 124.

Table 2-4

Estimates of the Labour Supply Elasticity for Young Workers, Based on Early Non-Experimental Microdata from the United States

				Elasticities		
	Data	Special characteristics	Estimation	Compensated wage	Income	Uncompensated wage
Study:						
Boskin [1973]	Survey of Economic Opportunity, 1967	White teenagers; not in school; 12 largest SMSAs	Imputed wages are instrumental variables			
		– Men		0.01	0.00	0.01
		– Women		0.43	0.00	0.44
Masters and Garfinkel [1977][1]	Survey of Economic Opportunity, 1967	20-24 years; not in school	OLS			
		– Married men		0.15	–0.14	0.01
		– Single men		0.26	–0.01	0.25
		– Single women		0.29	–0.00	0.29
		– Married women		0.66	–0.09	0.57

---

1 See Table 10-7, pp. 204-5. Dependent variable is hours (*HLF*).

of Economic Opportunity, as do many of the early studies, to demonstrate that labour supply estimates are "especially sensitive to the inclusion or exclusion of non-workers in the sample" for white husbands between the ages of 25 to 54. In particular, imputing the wages of non-workers and including them in the analysis increase the compensated and uncompensated wage elasticities for males. This result alone suggests that including female non-workers might increase the spread in wage elasticity estimates between males and females. But the elasticity estimates are also sensitive to the inclusion of control variables such as the level of education attained by the individual and the amount of family net worth. These findings inspire little confidence in the early results unless we understand the effects of such control variables. In many cases, however, the control variables appear to be thrown in to represent unobservable variables such as individual preferences. We suspect that they are poor proxy variables because they are not exogenous and are correlated with the labour supply error term. Thus better solutions are needed.

Although the bulk of early studies concentrate on individual labour supply behaviour for adults, there are a few studies that consider either cross-wage elasticities for adult family members or young workers. The results for cross-wage elasticities are summarized in Table 2-3. They show generally negative uncompensated and compensated cross-wage elasticities. McElroy [1981, p. 58] concludes, however, that "cross-wage elasticities are poorly estimated in both the male and female supply equations."

Results for young workers are more difficult to find. Age is often included as a control variable in labour supply equations to account for differences in behaviour over the life cycle, but it is not allowed to interact with the wage and income variables to estimate differences in the labour supply elasticities of young and adult workers. The evidence from two studies summarized in Table 2-4 suggests that, as for adults, females are more responsive than males in terms of wage elasticities. The estimates are within the range of those for the adult labour supply in Tables 2-1 and 2-2.

3 Second-Generation Analysis

Several important developments in labour supply analysis have occurred in the last decade. In this chapter we review these developments in order to consider what effect they might have on the consensus or stylized facts of labour supply behaviour established in the early literature.

The developments in the last decade may be characterized as either econometric refinements or data improvements. The development of better econometric techniques has certainly improved the analysis of labour supply behaviour. We suggest, however, that availability of better data has been equally, if not more, important, and that econometric refinements by themselves cannot hope to solve certain problems, despite the impressions created by the published technical literature. Emphasis in the published literature on applied econometric techniques should not make us ignore the contribution that better data have made to understanding labour supply behaviour. It is actually the integration of newer econometric procedures with superior data that yields higher-quality empirical analysis of labour supply behaviour.

We develop this argument in the next two sections. First, we consider econometric refinements, in particular the treatment of non-workers and sample selection bias, functional form, and non-linear and non-convex budget constraints. We then consider data improvements, in particular non-experimental and experimental panel data. These types of data have permitted researchers to address such problems as omitted variables (the worker heterogeneity question), to improve measurement of crucial variables, and to simplify budget constraints through experimental design. Finally, we consider developments in the analysis of labour supply in the presence of rationing and labour supply over the life cycle.

Econometric Refinements

Killingsworth [1983] provides a detailed survey of the econometric refinements of the late 1970s and early 1980s and makes the following distinction: "Most first-generation research either ignored or else dealt in an ad hoc fashion with a variety of problems of estimation. . . . In contrast, second-generation research tackled these problems head-on" [p. 131]. Our task in this section is to summarize the main elements of Killingsworth's argument, bring it up to date, and link it to emerging Canadian labour supply research.

Consider the basic labour supply model from equation 2.3 in Chapter 2. First-generation estimates often ignored those who did not report any hours of work ($h_1 = 0$ or $h_2 = 0$). When cases of zero hours were considered, such as in, for example, Hall [1973] or Boskin [1973], the analysis did not deal adequately with the fundamental problems introduced by the inclusion of non-participants. Boskin treated labour force participation (either $h = 0$ or $h > 0$) and hours of work decisions (h if $h > 0$) separately using ordinary-least-square (OLS) regression. Hall combined the non-participant ($h = 0$) and participant ($h > 0$) samples using OLS regression. Both authors used an imputed wage equation of the form

$$w_i = g_i(X_i) + e_i \qquad \text{for adult } i \ (i = 1, 2), \tag{3.1}$$

where X_i consists of observable personal characteristics. The regression results of equation 3.1 for participants, where w_i is observed, can then be used to impute wages for non-participants, where w_i is not observed, as well as participants. The imputed wage is then included in a standard stochastic version of labour supply equation 2.3:

$$\begin{aligned} h_i &= f_i(w_1, w_2, y) + u_i > 0, \text{ if } s_i = w_i, \\ &= 0, \text{ if } s_i > w_i \qquad (i = 1, 2). \end{aligned} \tag{3.2}$$

The works of Heckman [1976, 1979] and Lee [1978] demonstrate that the OLS regression estimator is biased in these circumstances. Wage equation 3.1 provides a classic example. The wage is observed only when hours of work are positive, but this is not likely to be a random sample of adults according to equation 3.2. That is,

$$\begin{aligned} E[w_i \mid h_i > 0] &= E\,[g_i(X_i) + e_i \mid u_i > -f_i(w_1, w_2, y)] \\ &= g_i(X_i) + E\,[e_i \mid u_i > -f_i(w_1, w_2, y)] \\ &\neq E\,(w_i), \end{aligned} \tag{3.3}$$

because $E[e_i \mid u_i > -f_i(w_1, w_2, y)] \neq 0$. The simplest argument is that unobserved characteristics, such as the degree of motivation, which increases hours worked, are also likely to be associated with higher wages so that e_i and u_i are positively correlated.

The standard solution to this problem is to specify a reduced-form participation equation to correct for sample selection bias. In linear form this can be specified as:

$$w_i = X_i B_i + e_i, \tag{3.4}$$

$$s_i^* = S_i A_i + r_i \equiv s_i, \text{ when } h_i = 0, \tag{3.5}$$

$$I_i = w_i - s_i^*$$

$$= X_i B_i - S_i A_i + (e_i - r_i) \equiv Y_i D_i + v_i, \tag{3.6}$$

$$P_i = 1, \text{ if } I_i > 0,$$

$$= 0, \text{ if } I_i \leq 0, \text{ and} \tag{3.7}$$

$$h_i = Z_i C_i + u_i > 0, \text{ if } P_i > 1,$$

$$= 0, \text{ if } P_i = 1, \tag{3.8}$$

where equation 3.5 determines the shadow entry wage and equations 3.6 and 3.7 determine labour force participation. If r_i and e_i are normally distributed, then equations 3.6 and 3.7 represent a probit model. Equation 3.3 becomes:

$$E[w_i \mid h_i > 0] = X_i B_i + E[e_i \mid v_i > -Y_i D_i]. \tag{3.3a}$$

This model makes the positive correlation between the two errors explicit, since $v_i = e_i - r_i$. If e_i and v_i are jointly normally distributed, then

$$E\,[w_i \mid h_i > 0] = X_i B_i + \left(\frac{\rho_{ev}}{\sigma_v}\right)\lambda_i, \tag{3.9}$$

where ρ_{ev} is the covariance between e_i and v_i, σ_v is the standard deviation of v_i, and

$$\lambda_i = n\left(\frac{-Y_i D_i}{\sigma_v}\right) \Big/ \left\{1 - N\left(\frac{-Y_i D_i}{\sigma_v}\right)\right\},$$

where $n(\cdot)$ is the standard normal density function and $N(\cdot)$ is the normal distribution function. The sample selection problem is equivalent to the omission of a relevant but unobserved variable, λ_i, unless $\rho_{ev} = 0$. Heckman's [1979] solution is to estimate the probit model represented by equations 3.6 and 3.7 to obtain consistent estimates of D_i and σ_v. This permits calculation of a consistent estimate of λ_i that can then be included as an instrumental variable in equation 3.9 to provide consistent estimates of B_i and ρ_{ev} by OLS regression. The wage offers estimated from equation 3.4 are then included in labour supply equation 3.8 as elements of Z_i to replace both the observed and unobserved actual wage offers.

Estimation of the labour supply equation presents a related problem. Since hours worked are truncated at zero hours, u_i must also be truncated and the OLS regression is therefore biased. The first solution was to specify a Tobit model [Tobin, 1958; Amemiya, 1973] of the form:

$$h_i = Z_i C_i + u_i,$$

$$H_i = h_i, \text{ if } h_i > 0,$$

$$= 0, \text{ if } h_i \leq 0, \tag{3.10}$$

where h_i represents an index of preferred hours of work and H_i represents observed hours worked. Since h_i is not truncated, u_i is assumed to be normally distributed, and equation 3.10 can be estimated by maximum likelihood (ML).

The Tobit model assumes that the hours variable can decline continuously to zero. In the presence of fixed costs of working [Cogan, 1981] this may be a poor assumption. One way to address this problem is to estimate labour supply equation 3.8 only for those with positive hours of work and to account for sample selection bias in the same fashion as in the wage equation. In other words,

$$E[h_i \mid h_i > 0] = Z_i C_i + E[u_i \mid v_i > -Y_i D_i]$$

$$= Z_i C_i + \left(\frac{\rho_{uv}}{\sigma_v}\right)\lambda_i, \tag{3.11}$$

where λ_i is estimated as in the wage equation. The estimates for equations 3.10 and 3.11 should coincide if the assumptions of the Tobit model are correct [Heckman, 1979]. Thus the estimation procedure in equation 3.11 is more general, although fixed labour supply costs are incorporated in an ad hoc fashion rather than by means of an explicit structural model. Numerous variations on these basic issues are available in the literature. Killingsworth [1983, pp. 130-68] provides a more thorough review.

A second econometric refinement involves the treatment of non-linear budget constraints arising from the tax-transfer system. Contrary to the popular classroom model, the effective marginal tax rate is neither constant nor monotonically increasing with earnings. This is because of such features as tax credits, exemptions, and provisions from social assistance programs. Consider the following two common situations. First, a married female in Canada who works and whose husband also works steadily will see the spousal exemption of her husband reduced \$1 for each \$1 earned. She effectively pays her husband's marginal tax rate on her initial earnings for the household. Once the exemption limit is reached, she reverts to the lower marginal tax rate of her personal income tax schedule. A second common situation concerns social assistance recipients. These individuals face high benefit reduction rates on initial earnings before reaching the more moderate marginal tax rates provided by the personal income tax system. Chart 3-1 illustrates these two cases for Winnipeg in 1976 for a married female with two children and a single mother with two children.[1]

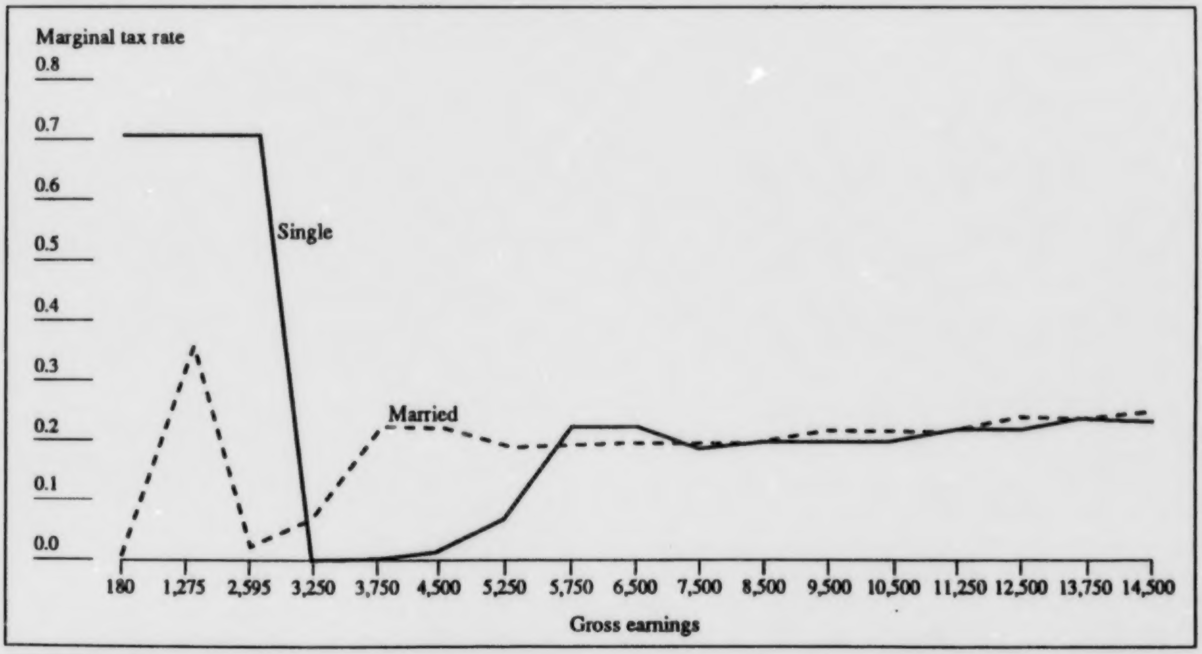
A non-linear budget constraint raises enormous econometric problems because the after-tax wage rate now depends upon earnings, and thus upon hours worked. One approach is to modify Hall's [1973] procedure of linearizing the budget constraint by using instrumental variables for the marginal after-tax wage and the concept of virtual income to obtain consistent labour supply estimates [Johnson and Pencavel, 1984; Leuthold, 1985]. This approach is inadequate, however, if the budget constraint is kinked and/or non-convex as in Chart 3-1. If kinked, the marginal after-tax wage is simply not defined, and when the budget is non-convex, a point of tangency may not be a global optimum.

To solve the problem properly, Burtless and Hausman [1978] use the indirect utility function to search for the optimal (utility-maximizing) hours worked among all budget segments. Estimates of the parameters of the indirect utility function obtained from iterative maximum-likelihood estimation can be directly translated into estimates of the parameters of the corresponding labour supply function using Roy's identity [Stern, 1986]. Hausman [1980] extends this approach to deal with non-workers, which is a particularly common situation in the case of married women.

The approach of Burtless and Hausman is not without problems. Killingsworth [1983, pp. 177-79] argues that econometric implementation of this complete-budget-constraint approach may be difficult because preferences, observed hours of work, and the specified budget constraint may all include errors that severely complicate the estimation procedure. Burtless and Hausman [1978, p. 1111] also note that application of Roy's identity is much more complex for the three-good case required by family labour supply models. Heckman [1981] also wonders whether the linearized budget constraint may not be more robust in practice. The only obvious way of answering this question is by direct comparison of the estimation techniques using the same data set. The only study of this type that we are aware of is by Moffitt [1984], who finds that the complete-budget-constraint approach provides lower estimates of the labour supply elasticity than the linearized-budget-constraint approach for older women in the United States.

Chart 3-1

Marginal Tax Rate Structure for Women, Manitoba, 1976



Hausman [1980] emphasizes that the choice of the parametric form of the direct or indirect utility function is no more restrictive than the choice of the parametric form of the labour supply function, since any well-behaved labour supply function must have corresponding direct and indirect utility functions. Stern [1986] provides a thorough review of this issue. Burtless and Hausman [1978] and Hausman [1980] start with the conventional linear labour supply function and then generate the corresponding direct and indirect utility functions to be estimated. Zabalza [1983] and Stelcner and Smith [1985], on the other hand, start out by specifying a CES utility function to be estimated and then derive the implied non-linear labour supply function. The issue, then, is whether the utility function to be estimated represents a good approximation to the data just as in the case when a particular labour supply function is specified and estimated.

We are not aware of any systematic investigation of the issue of functional form in labour supply regression. This is perhaps surprising in view of the considerable discussion of the possibility of a backward-bending, and thus non-linear, labour supply schedule. The issue may be important in the design of income maintenance programs if the labour supply behaviour of transfer recipients is different from the behaviour of the rest of the labour force. Most studies, however, report elasticity estimates for the mean characteristics of the sample or impose a constant-elasticity labour supply function on the data.

Labour Supply Results from Cross-Sectional Microdata

There have been other econometric refinements to the analysis of labour supply in recent years. The previous section concentrated on the two major issues that have received repeated attention – the treatment of non-workers under sample selection bias and the complications of non-linear and non-convex budget constraints. Most published second-generation econometric studies of labour supply address one or both of these issues.

It is not surprising to discover that most of the attention has been directed towards female labour supply, particularly married women. First-generation evidence suggested that both the compensated and uncompensated wage elasticities were larger for women than men and that male labour supply was quite inelastic. Hence, work disincentives associated with changes in the tax-transfer system were expected to affect mainly women, thereby justifying further investigation of their behaviour. Additionally, the two major issues addressed by the second-generation research were more important for the analysis of female labour supply than male labour supply. Problems arising from non-workers and selection bias are concentrated among married women, since men and single women have very high labour force participation rates, and the most severe problems arising from non-linear budget constraints, in particular non-convexity, are also expected to be concentrated among married women and among single mothers because of the design of the tax-transfer systems in the United States and Canada.

Tables 3-1, 3-2, and 3-3 provide information on second-generation empirical labour supply studies for U.S. and Canadian men, U.S. women, and Canadian women, respectively. We were able to find only two studies of male labour supply in Canada [Ehnes and Simpson, 1983; Ham and Hsiao, 1984] and six for the United States. Studies of male labour supply have normally been conducted to allow researchers to ignore the major issues discussed above and concentrate on other issues: taxes and uncertainty in a dynamic model [MaCurdy, 1981, 1983]; older workers [Hanoch and Honig, 1983]; non-pecuniary compensation [Filer, 1986]; and interval data [Ham and Hsiao, 1984]. The exceptions are the studies of family labour supply behaviour by Ehnes and Simpson [1983], Ransom [1987*a*, 1987*b*], and Hausman and Ruud [1984]. As a result it is very difficult to compare recent studies of male labour supply. The results in Table 3-1 for the United States are nevertheless consistent with the low estimates of the labour supply elasticity for males in earlier studies (Table 2-1), except for MaCurdy's estimates that involve a small sample of 121 married, working men from the control group of the Seattle-Denver Income Maintenance Experiment.[2] Ignoring MaCurdy's results, the compensated wage elasticity ranges from –0.17 (three of the other eight-point estimates are inexplicably negative) to 0.19, with a mean of 0.01. The income elasticity ranges from 0 to –0.10, with a mean of –0.03. Thus the uncompensated wage elasticity is small, ranging from –0.19 to 0.17, with a mean of –0.03.

Ehnes and Simpson [1983] use the Mincome pre-experimental, or baseline, data from 1974 to estimate a family labour supply model for adult males. They compare estimates from first-generation techniques, using OLS regression and ignoring truncation problems, with estimates using the Tobit model. As Table 3-1 indicates, the wage and income elasticities become larger as the truncation issues are addressed. For OLS regression on those with positive hours worked, the gross wage elasticity is –0.21; for OLS regression on the full sample, it is 0.18; and for the Tobit regression on the full sample, it is 0.27. The compensated wage and income elasticities are quite large in comparison with other studies in Table 3-1, except for

Table 3-1

Estimates of the Labour Supply Elasticity for Adult Men, Based on U.S. and Canadian Cross-Sectional Microdata

	Data	Special characteristics[1]	Estimation[2]	Elasticities		
				Compensated wage	Income	Uncompensated wage
U.S. studies:						
MaCurdy [1983]	Seattle-Denver Income Maintenance Experiment, 1972-75	121 married, working men; dynamic model with taxes and uncertainty	Two-stage least squares; wages are endogenous	0.74 to 1.43	–0.74 to –0.43	. . .
Hanoch and Honig [1983]	Retirement History Survey, 1969-75	Married; over 58 years old in 1969	Three-stage least squares on wages, hours, and weeks; hours and wages adjusted for selection bias	0.19	–0.02	0.17
Hausman and Ruud [1984]	Panel Study of Income Dynamics, 1976	Couples	ML; non-linear budget; household	0.07	–0.10	–0.03
Filer [1986]	Quality of Employment Survey, 1977	Over 16 years old and working over 20 hours per week; non-pecuniary compensation included	– OLS; wages are exogenous – Two-stage least squares; wages are endogenous	–0.10 0.09	0.00 0.00	–0.10 0.09
Ransom [1987*a*]	Panel Study of Income Dynamics, 1977	Families; husbands aged 30-50 years	ML; endogenous switching; actual wage	–0.17[3] –0.05[4]	0.00[3] –0.02[4]	–0.19[3] –0.09[4]
Ransom [1987*b*]	Panel Study of Income Dynamics, 1977	Families; husbands aged 30-50 years	ML (simultaneous Tobit); actual hours	0.03[3] 0.04[4]	–0.03[3] –0.03[4]	–0.04[3] –0.04[4]
Canadian studies:						
Ehnes and Simpson [1983][5]	Mincome baseline, 1974	Adults under 59 years of age; family labour supply model	– Positive hours only – Wages imputed by OLS – Tobit hours; wages imputed by OLS	0.19 0.42 0.95	–0.40 –0.60 –0.68	–0.21 0.18 0.27
Ham and Hsiao [1984]	Census of Canada, 1971	25-54 years; worked in 1970	ML; closed interval for 49-52 weeks worked	0.12	–0.01	0.11

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1 Samples exclude the self-employed, farmers, and students.

2 Linear budget constraint assumed unless otherwise stated.

3 Non-working wives only.

4 Working wives only.

5 Tobit elasticity estimates from Simpson [1984].

Table 3-2

Estimates of the Labour Supply Elasticity for Adult Women, Based on U.S. Cross-Sectional Microdata

				Elasticities		
	Data	Special characteristics[1]	Estimation[2]	Compensated wage	Income	Uncompensated wage
Study:						
Heckman [1976]	National Longitudinal Survey, 1966	Whites; married; 30-44 years	– Wages and hours imputed by OLS; hours reduced form	1.48	–0.02	1.46^3
			– FIML/Tobit model	4.35	–0.04	4.31^3
Cogan [1980]	National Longitudinal Survey, 1961	Whites; married; 30-44 years	– Wages and hours imputed by OLS	1.17	–0.03	1.14^3
			– Wages imputed by OLS; Tobit hours	3.60	–0.10	3.50^3
			– FIML/Tobit model	2.91	–0.09	2.83^3
Heckman [1980]	National Longitudinal Survey, 1966	Whites; married; 30-44 years	– Wages and hours imputed by OLS; hours reduced form	2.26	0.00	2.26^3
			– Wages and hours adjusted for selection bias	1.47	0.00	1.47^3
Schultz [1980]	Survey of Economic Opportunity, 1967	Whites; married; 35-44 years	– Wages and hours imputed by OLS	0.19	–0.05	0.13^3
			– Wages and hours imputed by OLS; Tobit hours	0.83	–0.18	0.65^3
Hausman [1981]	Panel Study of Income Dynamics, 1975	Household heads	ML; non-linear budget; taxes are endogenous	0.68	–0.18	0.50
Cogan [1981]	National Longitudinal Survey, 1967	Whites; married; 30-44 years; entry costs	– FIML/Tobit model	2.64	–0.19	2.45^3
			– ML	0.68	–0.03	0.65^3
Nakamura and Nakamura [1981]	Census, 1970	– Married; 30-44 years; average of three age groups	Wages and hours adjusted for selection bias	0.06	–0.27	-0.22^3
		– Married; 25-59 years; average of seven age groups		0.07	–0.16	–0.10
Dooley [1982]	Census, 1970	Whites; married; 30-44 years; average of three age groups	Wages and hours adjusted for selection bias	8.07	–0.35	7.73^3

Ransom [1982]	Panel Study of Income Dynamics, 1976	Married; husbands aged 30-50 years	ML	0.48	–0.07	–0.41[3]
Hanoch and Honig [1983]	Retirement History Survey, 1969-75	Single; 55 years old and over in 1969; four panels	Wages and hours adjusted for selection bias; three-stage least squares for wages, hours, and weeks	0.21	–0.06	0.15
Hausman and Ruud [1984]	Panel Study of Income Dynamics, 1976	Married; husbands aged 25-55 years	ML; non-linear budget	1.12	–0.36	0.76
Moffitt [1984]	National Longitudinal Survey of Older Women, 1972	. . .	– ML – ML; wages are endogenous; non-linear budget	0.71 0.39	–0.28 –0.18	0.43 0.21
Yatchew [1985]	Panel Study of Income Dynamics, 1975	Married	ML; piecewise budget; taxes are endogenous; wages are instrumental variables	1.36	–0.89	0.47
Ransom [1987*b*]	Panel Study of Income Dynamics, 1976-78	Married; working; husbands aged 30-50 years	ML (simultaneous Tobit); actual wage	0.73	–0.16	0.70

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1 Samples exclude the self-employed, farmers, and students.

2 Linear budget constraint assumed unless otherwise stated.

3 Killingsworth and Heckman [1986], pp. 189-90.

Table 3-3

Estimates of the Labour Supply Elasticity for Adult Women, Based on Canadian Cross-Sectional Microdata

				Elasticities		
	Data	Special characteristics^1	Estimation^2	Compensated wage	Income	Uncompensated wage
Study:						
Nakamura et al. [1979]	Census, 1971	Married; 30-44 years; average of three age groups	Wages and hours adjusted for selection bias	–0.01	–0.13	–0.14^3
Fortin [1979]		Married; living in Quebec; with low incomes	. . .	0.67	–0.22	0.45^4
Nakamura and Nakamura [1981]	Census, 1971	– Married; 30-40 years; average of three age groups	Wages and hours adjusted for selection bias; taxes are endogenous	0.08	–0.25	–0.16^3
		– Married; 25-59 years; average of seven age groups		0.10	–0.25	–0.15
Robinson and Tomes [1985]	Quality of Life Survey, 1979	Single and married; average of hourly wage and hourly paid samples	– Hours imputed by OLS; actual wage	–0.21	0.00	–0.21^3
			– Hours imputed by OLS; wages are instrumental variables	–0.65	0.00	–0.65^3
			– Hours adjusted for selection bias; actual wage	–0.27	0.00	–0.27^3
Stelcner and Smith [1985]	Census, 1981	Married; 20-54 years; CES utility function	– ML (probit)	0.04	–0.01	0.03^3
			– Wages and hours imputed by OLS	0.59	–0.11	0.48
Stelcner and Breslaw [1985]	Survey of Consumer Finance, 1979	Married; 20-54 years; living in Quebec; tax illusion	– Wages and hours adjusted for selection bias	0.49	–0.09	0.40^3
			– Wages adjusted for selection bias (GLS)	0.50	–0.09	0.41^3
			– Wages and hours imputed by GLS	1.52	–0.24	1.28^3
Prescott et al. [1986]	Mincome baseline, 1974	With low incomes; gross wage and wage net of child-care costs	Wages and hours adjusted for selection bias			
			– Married; gross wage	5.83	–6.58	0.35
			– Married; net wage	7.51	–7.03	0.48
			– Single; gross wage	8.22	–7.48	0.74
			– Single; net wage	6.41	–5.93	0.48

Cloutier [1986]	Survey of Consumer Finance, 1982	Married; 20-54 years; weeks worked plus reference week hours; full-time and part-time workers are exogenous	Wages and hours adjusted for selection bias; tax illusion; taxes are endogenous			
			– Full-time workers	0.21	–0.03	0.19
			– Part-time workers	0.11	0.00	0.11
			– All workers	0.18	–0.02	0.17
Smith and Stelcner [1985]	Census, 1981	Married; 20-54 years	Wages and hours adjusted for selection bias; taxes are endogenous	0.22	–0.13	0.09

1 Samples exclude the self-employed, farmers, and students.
2 Linear budget constraint assumed unless otherwise stated.
3 Killingsworth and Heckman [1986], p. 191.
4 Stelcner and Breslaw [1985], p. 1064, note 19.

MaCurdy's study. Ehnes and Simpson's results imply that the male labour supply results may be sensitive to the choice of econometric model and that further investigation of male labour supply using recently developed econometric techniques is warranted.

The study by Ham and Hsiao using interval data on hours and weeks worked from the 1971 census finds modest elasticity estimates for males between the ages of 25 to 54. Using assumptions comparable to those implicit in other studies not restricted to interval data – in particular, the assumption of a closed interval between 49 and 52 weeks worked per year – Ham and Hsiao compute a compensated wage elasticity of 0.12, an income elasticity of –0.01, and an uncompensated wage elasticity of 0.11. These results are well within the range of estimates provided by previous studies in the United States (Tables 2-1 and 3-1).

The results for females show greater variation than for males. The U.S. studies listed in Table 3-2 provide an even wider range of estimates, at least for wage elasticities, than did Table 2-2. The compensated wage elasticity ranges from 0.06 to 8.07, with a mean of 1.62. The income elasticity is more uniform as before, ranging from 0 to –0.89, with a mean of –0.17. The uncompensated wage elasticity ranges from –0.22 to 7.73, with a mean of 1.45. Indeed, the mean elasticity is not very informative because of the wide range of estimates. Killingsworth and Heckman [1986, p. 185] note that a range of this magnitude makes it easy for authors of empirical papers on female labour supply to find comparable results to their own. Thus there would appear to be little consensus on female labour supply estimates from cross-sectional data.

The effect of the major econometric refinements on these estimates is difficult to determine. Heckman's [1976] early treatment of non-workers and selection bias using a full-information maximum-likelihood (FIML) approach increases the wage elasticity estimates substantially in comparison with OLS estimates. Heckman's [1980] later study on the same data set reverses this result, however. As mentioned earlier, there is little direct evidence to judge the effect of incorporating non-linear budget constraints into the analysis. The papers that do this [Hausman, 1981; Hausman and Ruud, 1984; Moffitt, 1984; Yatchew, 1985] provide estimates that are at neither extreme of the range.

The results for Canadian females in Table 3-3 can be described in a similar fashion. The compensated wage elasticity varies from –0.65 to 8.22 (four of the 20 estimates are inexplicably negative), with a mean of 1.68. Unlike the results from the United States, the range of estimates for the income elasticity is also wide, from –7.48 to 0, with a mean of –1.50. The uncompensated wage elasticity varies from –0.65 to 1.28, with a mean of 0.19.

This high variation is sensitive, however, to the inclusion of the results of Prescott et al. [1986] from the Mincome baseline cross-section.[3] These results provide much higher estimates of the compensated wage and income elasticities than other Canadian studies, many of which use similar estimation procedures (e.g., Nakamura et al. [1979] and Nakamura and Nakamura [1981] using the 1971 Canadian census; Stelcner and Breslaw [1985] and Cloutier [1986] using the Survey of Consumer Finance). If the results of Prescott et al. are excluded, the compensated wage elasticity ranges from –0.65 to 1.52, with a mean of 0.20; the income elasticity, from –0.25 to 0, with a mean of –0.10; and the uncompensated wage elasticity, from –0.65 to 1.28, with a mean of 0.11.

Killingsworth and Heckman [1986, p. 185] note that a number of recent studies have generated backward-bending supply curves for adult females (negative uncompensated wage elasticities). What they fail to mention is that these studies [Nakamura et al., 1979; Nakamura and Nakamura, 1981; and Robinson and Tomes, 1985] and other recent studies with very small wage elasticities [Smith and Stelcner, 1985; Stelcner and Smith, 1985] constitute a majority of the estimates for Canadian microdata. If we include Cloutier's [1986] result in this group, only Fortin [1979], Stelcner and Breslaw [1985], and Prescott et al. [1986] provide large wage elasticity estimates typical of results from the United States. The majority of the Canadian results, then, seem at odds with the U.S. results that female labour supply is more sensitive to wage rate variation. The results for most of the studies suggest labour supply behaviour for adult females that is not very different from that of U.S. adult males.

Killingsworth and Heckman [1986, p. 193, note 32] suggest that these results may be explained by differences in the samples and model specifications used in these studies. Little can be said until the issue is systematically examined using various samples, model specifications, and estimation procedures on a single data set. We return to this question in Chapter 4.

The results in Table 3-3 are the first phase of studies using Canadian microdata, but they fit into the second generation of analysis, all adopting methods to correct for selection bias in the hours equation at least. Robinson and Tomes [1985] find that correction for selection bias does not affect their estimates very much, but their inexplicably negative compensated wage elasticities suggest caution in relying heavily on their results. Studies with non-linear and non-convex budgets (or endogenous taxes, i.e., Nakamura and Nakamura [1981]; Smith and Stelcner [1985]; and Cloutier [1986]) find uniformly low elasticity estimates in comparison with their U.S. counterparts. Smith and Stelcner and Cloutier also find evidence of tax illusion (incorrect perception of marginal and/or average tax rates) for adult females.

Finally, we return to the questions of family and youth labour supply initiated in Chapter 2. Table 3-4 presents results on family labour supply. Only uncompensated cross-wage elasticities have been reported in the few studies available. These results are uniformly small and insignificant for husbands (with respect to the wife's wage) and generally negative for wives. Hausman and Ruud [1984] find a large, significant cross-wage elasticity when they account for the non-linear and non-convex budget constraints facing wives. Prescott et al. [1986] also find a significant negative elasticity using their net-wage concept, which deducts child-care expenses from the gross wage. Using the gross wage, the elasticity remains negative but is insignificant, while Ransom [1987*b*] also obtains an insignificant estimate. The results are consistent with earlier estimates (Table 2-3) and, except for Hausman and Ruud's estimate for wives, fall within a very small range. The number of studies is limited, however, and the econometric analyses are not comparable.

Table 3-4

Estimates of the Uncompensated Cross-Wage Elasticity for Family Labour Supply, United States and Canada

	Husbands	Wives
Study:		
United States		
Hausman and Ruud [1984]	0.00	–2.36
Ransom [1987*a*]	0.00	. . .
Ransom [1987*b*]	–0.03	–0.21
Canada		
Prescott et al. [1986]		
Gross wage	. . .	–0.19
Net wage (net of child-care costs)	. . .	–0.20
Baffoe-Bonnie [1989][1]		
Primary sector	–0.01	–0.05
Secondary sector	0.01	0.03

[1] Baffoe-Bonnie uses the General Segmentation Survey, 1979, conducted at Dalhousie University. Primary and secondary workers are determined exogenously by the industrial classification of the establishment. Estimation is done by three-stage least squares with adjustment for selectivity bias for wages.

There do not appear to have been any published studies of teenage (15 to 19 years) or youth (15 to 24 years) labour supply using microdata since the early literature (Table 2-4). Goldfarb and Yezer [1983] find a backward-bending labour supply curve for U.S. teenagers, but they rely upon the Standard Metropolitan Statistical Area as their unit of observation. The extent and nature of the aggregation biases from this approach is unknown.

Data Refinements: Non-Experimental and Experimental Panel Data

The previous two sections examined recent econometric refinements that resulted in a new wave, or a second generation, of labour supply results. Another important development, however, has been the advent of panel data in which repeated observations on a cross-section of individuals are taken over time. This includes several non-experimental panel data sets in the United States (most notably, the National Longitudinal Survey and the Panel Study of Income Dynamics) and panel data sets for each of the income maintenance experiments in the United States (most notably, the New Jersey and Seattle/Denver experiments) and Canada (Mincome).

Panel data has permitted economists to address several issues. Perhaps the most important of these has been the difficulties related to omitted variables. Estimates arising from cross-sectional data are biased if omitted variables are correlated with included regressors. Thus differences in individual motivation may affect labour supply, wages, and unearned income (not to mention education, net worth, and other variables typically included in labour supply regression models), imparting bias to labour supply elasticity estimates. Since a variable such as "individual motivation" is difficult to measure, and hence often referred to as an unobservable individual difference, it cannot be included in analyses of cross-sectional data in any satisfactory manner.

One solution to the omitted variable bias arising from unobserved individual differences is to analyse an individual's behaviour with reference to his/her past behaviour [Hsiao, 1986; Chamberlin, 1978; Heckman, 1981]. In effect, this means that unobserved individual differences are controlled by comparing changes in individual behaviour over time rather than by comparing differences in the behaviour of individuals at the same point in time. Information on past labour supply is used to assess current labour supply in order to eliminate the omitted variables bias in the labour supply equation.

This approach requires the assumption that relevant unobserved individual characteristics change slowly relative to critical observed variables such as wages and income. Unfortunately, these critical observed variables change slowly as well in most non-experimental circumstances. Therefore, this lack of variability makes it difficult to obtain precise estimates of the labour supply elasticity.

One solution to the problem of inadequate non-experimental variation in the critical labour supply variables is to introduce variation experimentally.[4] The income maintenance experiments were designed precisely for this purpose. Despite certain design limitations and flaws, the experiments remain a valuable data source for the analysis of labour supply behaviour. They provide panel data in which the critical labour supply variables from an economic standpoint – namely, after-tax wages and unearned income – contain substantial individual-specific variation.

Panel data are also useful in other ways. They may permit better measurement of variables. Cross-sectional microdata sets, particularly in Canada, have measured hours worked during some brief reference period (the Canadian census and the Survey of Consumer Finance, for example). One must then combine the number of hours of work reported for the reference week with the number of weeks worked during the year to obtain a rough estimate of labour supply in terms of annual hours worked. In contrast, panel data may have more detailed information on hours worked during the entire period between interviews. Mincome, for example, collected detailed information on the number of hours worked on a continuous basis in order to capture annual hours worked much more accurately. More accurate information on hours worked also reduces the error in determining gross wages for salaried workers. Concepts such as permanent or normal income can be approximated more closely from a series of observations over time than from a single observation in a cross-sectional data set.

Experimental panel data can also eliminate the problems that arise when budget constraints are non-linear and non-convex. If the major source of variation in wage rates is introduced experimentally through tax rate changes, and if these tax rate changes introduce a linear budget constraint (a single tax rate), then the complex problem of non-linear and non-convex budget constraints can be sidestepped, allowing researchers to concentrate on other issues. Since correction for this problem is both complex and controversial, this is a useful simplification.

Panel data sets are not without their own problems, however. Stafford [1986, pp. 411-13] provides a brief list of the problems with non-experimental data: measurement error (which may or may not be greater than for cross-sectional data); falling panel response rates through attrition; and changes in questionnaire wording and layout over time. To these we could add the special problems of experimental panel data, that is, non-random sampling procedures and sample truncation. These issues will be considered further in Part B. It is sufficient to note at this point that there is no ideal data set or method of analysis. Each data set and method involves strengths and weaknesses, some of which may be assessed in terms of their effects on the estimation of labour supply parameters and some of which cannot.

Results from Studies Using Panel Data

We have already listed some studies using panel data sets because in some cases [e.g., MaCurdy, 1983] these sets were simply used as if the data were cross-sectional, thereby ignoring the helpful features of panel data discussed in the previous section. In this section, we consider studies that do take advantage of the longitudinal nature of the data in trying to address problems in labour supply estimation.

First, we consider studies using non-experimental panel data. From the standpoint of estimating the elasticities of the static labour supply model, the results using non-experimental panel data are disappointing. Many published studies used non-experimental panel data to investigate particular aspects of labour supply behaviour: for example, Heckman and MaCurdy [1980], MaCurdy [1981, 1983], Altonji [1986], and Jakubson [1988] on life-cycle labour supply; Ham [1986] on unemployment as intertemporal labour supply behaviour; Blau and Robins [1986] on labour force status in response to welfare programs; Johnson and Skinner [1986] on labour supply and marital separation; and Jakubson [1988] and Lundberg [1988] on the influence of children on labour supply. The studies on life-cycle labour supply estimate the intertemporal substitution elasticity, to be considered in the next section, but do not provide estimates of the static labour supply elasticities that were the focus of earlier empirical work.

There are two major reasons for the lack of static elasticity estimates from non-experimental data. The first is that the availability of panel data shifted concern from estimating the static labour supply model to the estimation of the life-cycle model. The latter can be considered an important advancement in labour supply analysis, particularly in terms of theoretical understanding. Killingsworth [1983, pp. 214-20] argues that only unanticipated changes in wage rates and income can be understood in terms of the static labour supply elasticities. Anticipated changes, arising from dynamic equilibrium behaviour over the life cycle, cannot be analysed in the static model. Moreover, static models may confuse unanticipated (or permanent) changes with anticipated (or life-cycle) changes if they are misspecified. In terms of Killingsworth's classification, life-cycle models may well constitute the third generation of empirical studies of labour supply. We return to this issue in the next section.

The second reason for the lack of static elasticity estimates from non-experimental data is that there is usually very little unanticipated variation in wage rates and income. In the absence of significant reforms to the tax-transfer system, the after-tax wages and incomes of individuals tend to change slowly and are very likely dominated by anticipated variation over the life cycle. Estimates of labour supply response to wage and income changes will be imprecise under these conditions. Moreover, if we are interested in the impact of reforms to the tax-transfer system that are permanent and unanticipated, these estimates may be quite inaccurate.

The income maintenance experiments were designed to deal directly with these problems. By introducing large exogenous (unanticipated) changes in the tax rates and income guarantee levels of selected participants (the treatment group), and by monitoring the labour supply behaviour of these participants and a control group before and after the tax and income changes, the experiments provide data that can overcome some of the complex econometric problems facing the estimation of static labour supply models. Thus analysis of the experiments can provide additional information on labour supply behaviour.

The simplest method of analysis of the experiments is a direct comparison of the changes in the labour supply behaviour of the treatment and control groups using analysis-of-variance (ANOVA) models [Keeley, 1981, pp. 129-35]. In terms of the general linear model familiar to most economists, these models are of the general form

$$h = Xb + Ta + u, \tag{3.12}$$

where X is an array of control variables. Among the control variables may be included a measure of pre-experimental labour supply.[5] Further,

$T = 1$, if assigned to an income maintenance program (treatment group);
$= 0$, if not (control group).

T may be expanded to an array of dummy variables, some of which identify specific plans, or other dummies that identify differences in the income guarantee level and tax rates among plans. Results from this method of analysis provide estimates of labour supply response to a particular array of programs, each consisting of an individual tax rate and a family income guarantee level, in comparison to the tax-transfer system prevailing at that time. In this analysis, truncation of the sample at a specified income level does not pose a problem since it is part of the design of the specific policy alternative being compared. (Other problems, such as finite program duration and non-random assignment of participants to plans, cannot be disregarded even in this simple analysis.) Yet such analysis may not be particularly useful to the social policy debate. Program comparisons from the 1970s are unlikely to be particularly useful to debates in the 1990s. The tax-transfer system has changed and the specific negative-income-tax features of past experiments may not be seriously considered in the future. In short, the ANOVA results are not useful for general application to social policy analysis. We require labour supply elasticity estimates for this task [Keeley, 1981, Chapter 3]. For this reason we focus on the labour supply elasticity estimates provided by the income maintenance experiments in comparison with earlier results using non-experimental data.

Again, we are fortunate to have previous surveys of the labour supply results from the four income maintenance experiments in the United States by Keeley [1981, Chapter 5], Moffitt and Kehrer [1981], Killingsworth [1983, Chapter 6], Basilevsky and Hum [1984], and Robins [1985]. Tables 3-5 and 3-6 summarize the results provided by these surveys for adult males (married men only) and adult females (married women and single female heads) and add any results published since the surveys appeared.

Despite the attention the experiments have received from economists and other social scientists, the results in Tables 3-5 and 3-6 do not provide a systematic overview of the experimental findings in terms of elasticity estimates. Many of the early studies concentrated on ANOVA models typified by equation 3.11 and ignored structural labour supply models and their associated elasticity estimates. This immediately limits the comparability of the results since the experiments differed in terms of generosity. Furthermore the differences are difficult to quantify since each experiment involved several plans (tax rates and income guarantee levels).[6]

Structural labour supply models and elasticities were not estimated routinely until the final and largest experiment in Seattle and Denver. Most of the results in Tables 3-5 and 3-6 refer to this experiment. The basic model appeared in Keeley et al. [1978] and explained the difference between experimental and pre-experimental labour supply by the change in after-tax wages, incomes, and other control

Table 3-5

Estimates of the Labour Supply Elasticity for Adult Men, Based on Experimental Panel Data from the United States

	Data	Special characteristics[1]	Estimation[2]	Elasticities		
				Compensated wage	Income	Uncompensated wage
Study:						
Watts et al. [1974]	New Jersey-Pennsylvania	Whites; married; intact;[3] two least generous plans deleted	OLS; no structural model	–0.09	0.06	–0.06^4
Hausman and Wise [1976]	New Jersey	Household heads	ML; adjusted for income truncation bias; wages and hours are endogenous	0.16	–0.02	0.14
Bawden [1977*a*]	Rural communities	Whites; married	OLS	0.01	0.00	0.01^5
Watts and Horner [1977]	New Jersey	Married; Cobb-Douglas utility function	OLS	0.08	–0.04	0.05^5
Ashenfelter [1978]	Rural communities	Married	OLS	0.13	0.00	0.13^5
Burtless and Hausman [1978]	Gary	Married	ML; non-linear budget constraint	0.08	–0.07	0.00^5
Keeley et al. [1978]	Seattle-Denver	Married	Tobit linear budget; break-even variables	0.10	–0.13	0.02^4
Moffitt [1979]	Gary	Blacks; married; negative income tax effect only	Tobit linear budget; using average marginal tax rate	0.02	–0.05	–0.01^4
Robins and West [1980]	Seattle-Denver	Married	Tobit linear budget; break-even variables; includes the probability of being below the break-even point	0.12	–0.08	0.06^4
Burtless and Greenberg [1982]	Seattle-Denver	Married	Tobit linear budget; break-even variables – Three-year experiment – Five-year experiment	0.11 0.06	–0.06 –0.30	0.08^4 –0.12^4
Johnson and Pencavel [1984]	Seattle-Denver	Married; spouses working	Non-linear least squares; selectivity bias corrected for exclusion of			

			non-workers; dynamic Stone-Geary utility function			
			– Short run	0.13	–0.10	–0.06
			– Long run	0.19	–0.08	0.02
Robins [1985]	– New Jersey-Pennsylvania	– Simple average of reported estimates for husbands	Various methods and models	0.09	–0.02	0.07
	– Rural communities			0.09	0.00	0.09
	– Gary			0.06	–0.08	–0.02
	– Seattle-Denver			0.09	–0.14	–0.05
	– All experiments	– Average of above weighted by sample size		0.08	–0.10	–0.02

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1 Samples exclude the self-employed, farmers, and students.

2 Linear budget constraint assumed unless otherwise stated.

3 Intact means no change in marital status during the experimental period.

4 Killingsworth [1983], pp. 398-99.

5 Moffitt and Kehrer [1981].

Table 3-6

Estimates of the Labour Supply Elasticity for Adult Women, Based on Experimental Panel Data from the United States[1]

		Elasticities					
		Married			Single family heads		
	Data	Compensated wage	Income	Uncompensated wage	Compensated wage	Income	Uncompensated wage
Study:							
Bawden [1977*b*]	Rural communities	0.01	0.00	0.01[2]	...	...	...
Cain et al. [1977]	New Jersey	0.22	–0.30	–0.08[2]	...	...	...
Ashenfelter [1978]	Rural communities	0.42	0.02	0.43[2]	...	...	...
Keeley et al. [1978]	Seattle-Denver	0.22	–0.95	0.00	0.12	–0.32	–0.03[3]
Moffitt [1979]	Gary	0.07	0.07	0.16	–0.04	–0.23	–0.12[3]
Hausman [1980]	Gary				0.10	–0.07	0.03[2]
Robins and West [1980]	Seattle-Denver	0.30	–0.61	0.13	0.09	–0.24	–0.05[3]
Burtless and Greenberg [1982]	Seattle-Denver						
	– Three-year experiment	0.37	–0.78	0.19	0.17	–0.26	0.05[3]
	– Five-year experiment	–0.05	–1.32	–0.36	0.01	–0.46	–0.23[3]
Johnson and Pencavel [1984]	Seattle-Denver						
	– Short run	0.16	–0.11	0.08	0.15	–0.11	0.07
	– Long run	0.18	–0.07	0.12	0.29	–0.12	0.20
Robins [1985]	New Jersey-Pennsylvania	–0.08	–0.28	–0.36	...	...	...
	Rural communities	0.28	0.01	0.29	...	...	...
	Gary	0.37	0.26	0.63	0.14	–0.20	–0.06
	Seattle-Denver	0.14	–0.12	0.02	0.12	–0.15	–0.03
	All experiments	0.17	–0.06	0.11	0.13	–0.16	–0.03

[1] Samples exclude the self-employed, farmers and students. Linear budget constraint assumed unless otherwise stated.

[2] Moffitt and Kehrer [1981].

[3] Killingsworth [1983], pp. 398-99.

variables during the same period. This approach corrects for the omission of unobserved (but stable) person-specific differences that may bias cross-sectional estimates. This model is discussed in considerable detail in Part B. Robins and West [1986] and Burtless and Greenberg [1982] use a similar approach to Keeley et al. Johnson and Pencavel [1984], on the other hand, develop a dynamic labour supply model from a dynamic Stone-Geary utility function, in which the current preferences of the household depend upon previous labour supply and consumption behaviour. Their results indicate that past behaviour conditions current behaviour (state dependence), which implies that cross-sectional results are necessarily doubly biased because of unobserved person-specific variables (heterogeneity) and state dependence. Their elasticity estimates are similar to other results in Tables 3-5 and 3-6, however.

Robins [1985] summarizes the elasticity estimates for the four U.S. experiments. His findings are included at the bottom of Tables 3-5 and 3-6. The immediate impression one obtains from these results in comparison with the earlier results from non-experimental cross-sectional data is that they contain less variability and are uniformly quite low. The results in Table 3-5 reinforce earlier findings for adult males from non-experimental cross-sectional data. The compensated wage elasticity varies from –0.09 (the only negative result and one that is not derived from a structural labour supply model) to 0.19. Robins' mean estimate is 0.08. The income elasticity ranges from 0.06 to –0.30 (again the estimate by Watts et al. is the only positive result). Robins' mean estimate is –0.10 and his mean estimate of the uncompensated wage elasticity is –0.02, or very slightly backward-bending. In fact, there are as many positive as negative estimates of the uncompensated wage elasticity in Table 3-5, but none exceed 0.12 in absolute value.

The results for adult females in Table 3-6, particularly married women, are more uniform than previous results from non-experimental cross-sectional data. The compensated wage elasticity for married women varies only from –0.08 to 0.42, with Robins' mean estimate at 0.17. The income elasticity varies from 0.26 to –1.32, with Robins' mean being –0.06. The uncompensated wage elasticity is generally positive and Robins' mean estimate is 0.11. For single female household heads, the results are closer to those found for husbands in Table 3-5. The compensated wage elasticity varies from –0.04 to 0.29, with Robins' mean being 0.13. The income elasticity varies from –0.07 to –0.46, with Robins' mean being –0.16. The uncompensated wage elasticity ranges from –0.23 to 0.20, with Robins' mean estimate at –0.03. Thus the compensated wage and income elasticities almost exactly offset each other for single female heads and for husbands, but these elasticity estimates are generally larger in absolute value for the former.

Keeley [1981, Chapter 5] and Robins [1985] note that the experimental elasticity estimates are more uniform, more consistent with economic theory in terms of signs, and generally lower in absolute value for married women. These results should be viewed cautiously in view of the small sample size of each experiment relative to non-experimental microdata samples and particularly the concentration on the Seattle-Denver sample. More results from the other experiments that could be directly compared to the Seattle-Denver results would be useful in confirming these impressions.

The analysis of the experiments must also continue to deal with a variety of issues that have been raised. These include the treatment of households enrolled in the experiment who receive no payments [Robins et al., 1983; Cogan, 1983; Keeley et al., 1977, 1978]; attrition [Hausman and Wise, 1979; Robins and West, 1986]; the effect of finite program duration [Robins, 1984];[7] and the role of dynamic labour supply models [Johnson and Pencavel, 1984]. It would be useful if, where possible, future analysis provided results (i.e., elasticity estimates) that could be compared to other labour supply results, both experimental and non-experimental. We return to this issue in Part B.

Life-Cycle Models

Attention has shifted in recent years from estimation of static labour supply models to estimation of dynamic labour supply models, particularly life-cycle models. Life-cycle models capture potentially important interrelationships among past, current, and future behaviour. In this section we briefly examine the issues raised by life-cycle models for the analysis of labour supply behaviour. Killingsworth [1983, Chapter 5] provides an extensive review of the issues.

Life-cycle models are considerably more complex than the static labour supply model presented in Chapter 2. For this reason they are usually restricted to individual labour supply and ignore household interaction. An individual maximizes lifetime utility, V, given by:

$$V = \sum_{t=1}^{T} (1+\rho)^{-t}\, U(g_t, h_t), \tag{3.13}$$

subject to the lifetime budget constraint

$$A_t = \sum_{t=0}^{T} (1+r)^{-t}\, (w_t h_t - g_t) = 0 \tag{3.14}$$

and

$$0 \le h_t \le H_m, \tag{3.15}$$

where

T = the planning period (expected remaining lifetime);
ρ = the individual rate of time preference;
U = the utility derived from consumption g_t and labour supply h_t at time t;
A_t = wealth at time t;
r = the real interest rate;
w_t = the wage rate at time t (the price of g_t is 1); and
H_m = the maximum time available for work.

An interior solution to this problem (a lifetime plan $[h_t, g_t, \lambda_t, t = 0, \ldots, T]$) is given by:

$$(1+\rho)^{-t} \frac{dU}{dh_t} = (1+r)^{-t}\, w_t \lambda_t \tag{3.16}$$

and

$$(1+\rho)^{-t}\ \frac{dU}{dg_t} = (1+r)^{-t}\ \lambda_t, \qquad (3.17)$$

where λ_t is the marginal utility of wealth at time t. Since dU/dh_t and dU/dg_t are only functions of current consumption g_t and current wage w_t, there might appear to be no additional difficulties, but this ignores λ_t and the lifetime budget constraint (equation 3.14). We can solve equations 3.16 and 3.17 to obtain:

$$h_t = h_t\ (w_t, \lambda_t, \rho, r) \qquad (3.18)$$

and

$$g_t = g_t\ (\lambda_t, \rho, r).^{8} \qquad (3.19)$$

If we want to eliminate the person-specific marginal utility of wealth, λ_t, however, we must use equation 3.14, in which case h_t depends upon wealth A_t (or past values of wages) and future values of wages. Inclusion of a wealth variable, A_t, appears to solve part of the problem, although A_t is likely to be endogenous if preferences are person-specific (see the discussion in Chapter 2). Thus λ_t neatly captures the effect of past and future wages on current labour supply arising from the lifetime budget constraint (equation 3.14), but it is unobservable.

Equation 3.18 explicitly introduces an unobservable person-specific determinant of labour supply that is correlated with w_t (and other parameters in a more fully developed model), introducing bias into estimates of the labour supply parameters in a cross-sectional study [Jakubson, 1988]. In more complex models such as one in which wages are endogenous, other person-specific effects, including the rate of time preference ρ and other elements of tastes, will introduce additional sources of bias as in Chapter 2.

Various methods have been used to estimate life-cycle labour supply models using panel, pooled, and even cross-sectional data (see Killingsworth [1983, pp. 263-97] and Altonji [1986] for a review of empirical methods). Of particular interest has been the intertemporal substitution elasticity, measuring the effect of the current wage on current labour supply holding the marginal utility of wealth constant $[d(\ln h_t)/d(\ln w_t) \mid \lambda_t]$. For a given equilibrium lifetime plan (represented by given λ_t), this elasticity tells us how labour supply changes in response to *anticipated* changes in wages over the lifetime. Economic theory predicts that this elasticity will be positive since an individual's rational lifetime plan is expected to involve more work when work is more profitable. Estimates of this elasticity are generally positive, ranging from –0.06 to 1.90 for adult men, with a mean estimate of 0.28, and ranging from 0.04 to 1.7 for adult women, with a mean of 0.75.[9]

Static labour supply models may easily confuse the intertemporal substitution elasticity, which arises from a wage change anticipated by the lifetime plan, with the compensated and uncompensated wage elasticities arising from an unanticipated wage change. In principle, life-cycle models involve no such confusion since the effects of an unanticipated wage change on the equilibrium path may be evaluated in terms of its effect on labour supply through changes in lifetime wealth (the "income effect") and its effect on labour supply through changes in the cost of leisure (the compensated wage effect). Hence life-cycle models can enrich our understanding of labour supply behaviour and the impact of social policy.

This does not mean that static labour supply analyses should be abandoned, only that their limitations should be carefully understood. Static labour supply models measure the permanent effect of unanticipated changes in wages and income on labour supply, precisely the effect that is required for analysis of social policy alternatives. Since static labour supply models are easier (but not easy!) to specify, estimate, and apply than life-cycle models, they may still be the most useful vehicle for this type of analysis. But static models cannot tell us how individual labour supply behaviour varies when unanticipated events – such as policy changes – do not occur. Indeed, this limitation of static models may also explain why emphasis has shifted to the estimation of the intertemporal substitution effect in labour supply. The non-experimental panel data contain some anticipated wage variation (the more the longer is the panel) but little unanticipated wage variation in the absence of dramatic policy initiatives. And dramatic policy changes are rare. Hence it is appropriate to try to estimate the intertemporal substitution elasticity but not the traditional static labour supply elasticities with such data. Accurate estimation of static labour supply elasticities requires rare dramatic policy changes or panel data in which such unanticipated changes are introduced experimentally. Experimental panel data, on the other hand, permit the estimation of both static and life-cycle models and both anticipated and unanticipated effects on labour supply.

Constrained Labour Supply

Labour supply in economic theory denotes the hours an individual is willing to provide to the market, or what we might term desired hours worked. Interpreting the effect of wage and income changes as labour supply changes requires the assumption that employment is not "rationed" or constrained by insufficient demand. If employment is constrained for some individuals, then actual hours worked is less than desired hours worked. Under these circumstances, the term hours worked as a measure of labour supply contains measurement error that biases estimates of the labour supply function. Thus we may rewrite any previous labour supply function as:

$$h^s = h^s(\cdot) = h + d \geq h, \tag{3.20}$$

where h^s is labour supply; h is observed hours worked; and $d = h^s - h \geq 0$ represents deficient employment demand.

Investigation of the bias arising from employment constraint is difficult because we do not have a direct measure of deficient employment demand in microdata sets. Two indirect measures of d have been used to investigate the problem in non-experimental cross-sectional and panel data: unemployment experienced during the survey period and reported differences between desired and actual hours worked. We provide only a brief discussion of the results of research into constrained labour supply. For a more complete review, see Ham [1988].

Ham [1982] compares the labour supply estimates for the entire cross-sectional sample of U.S. men, 25 to 50 years old, with the results when unemployed workers are excluded, correcting for sample selection bias in the latter case. He rejects the hypothesis that the labour supply coefficients are the same in each model, although the bias appears to be concentrated in the demographic variables rather than wages and income.

Ashenfelter and Ham [1979] and Ham [1980] use non-experimental panel data to probe this issue further. In particular, their studies use fixed-effect models to capture person-specific differences in preferences, which could account for the differences in labour supply behaviour between fully employed and underemployed workers found in Ham [1982]. Ham [1980] uses both reported unemployment and reported differences between actual and desired hours of work (or underemployment) to distinguish workers. He finds evidence that hours worked are overpredicted for unemployed workers whether or not they also report underemployment. Ashenfelter and Ham insert reported hours of unemployment directly into an earnings function and find it to be statistically significant.

Ham [1986] uses MaCurdy's [1981] intertemporal labour supply model that allows for uncertainty, but he adds a term to represent underemployment. This term is either a dummy variable to indicate whether the individual experienced unemployment or underemployment during the year, or reported hours of unemployment. The term is generally statistically significant, implying that the standard labour supply model that ignored constraints on employment is unspecified.

This approach to the issue of constrained labour supply has been criticized because it relies on the assumption that unemployment is exogenous. If unemployment is an aspect of labour supply choice, then it is not only endogenous but likely determined by the same variables that determine hours worked, ruling out any estimation procedures or tests involving instrumental variables [Kniesner and Goldsmith, 1987]. Yet there are no other proposed methods to investigate the issue.

The practical question at this point is the extent of bias that might arise from ignoring employment constraints on hours worked. Ham [1982] finds the uncompensated wage elasticity to be virtually the same for the full sample (–0.16) and the sample that excludes the unemployed (–0.14). The compensated wage elasticity is also negative (–0.05 for the full sample and –0.08 for the sample without the unemployed), contrary to the prediction of economic theory. Ham's [1986, Table 1] results for the life-cycle model appear more damaging in some cases. The introduction of an unemployment or underemployment dummy variable reduces the size of the intertemporal substitution elasticity, but it remains in its conventional range (small and positive). The introduction of unemployed hours, however, reduces the point elasticity estimate to virtually zero (–0.01 to 0.02). The unemployment and underemployment variables are uniformly negative and significant. Despite the problems of incorporating employment constraints into the labour supply function, Ham's results suggest that the issue deserves further attention.

Note, however, that the bias in labour supply estimates from employment constraints should be less serious for experimental data. The assignment of plans, which change after-tax wages and incomes, is likely to be uncorrelated with the probability that an individual faces employment constraints at a given site. Thus the estimates of the labour supply elasticity for any particular experimental site are not biased by the presence of these constraints, although comparisons across sites may be affected by differences in labour market conditions among sites (and hence the probability that an individual's hours worked are constrained among sites). This bias cannot be similarly dismissed for non-experimental data because wage and income changes are not exogenous and are likely to be correlated with the likelihood that employment is constrained. Thus studies of this issue have properly been concerned with the potentially more serious biases arising from non-experimental data.

The econometric refinements and data advances of the last two decades have been significant; yet it still must be acknowledged that large variations in the estimates of the labour supply elasticity still persist. The next chapter attempts to reconcile the conflicting results.

4 Reconciliation of the Labour Supply Results

Considerable academic effort and expertise have been devoted to the estimation of labour supply models, particularly the static neoclassical model of Chapter 2. From the standpoint of social policy questions as well as other analyses of the labour market effects of changes to the tax-transfer system, the value of this research lies in the provision of precise estimates of the critical labour supply parameters. As a result, the burgeoning research has been reviewed on a regular basis to extract consensus, or simply "best guess," estimates for applied economic and policy research. These reviews include many already cited for the United States (Keeley [1981], Killingsworth [1983], Robins [1985], for experimental evidence; Pencavel [1986], Killingsworth and Heckman [1986]).

Rea [1983] and Osberg [1986] have reviewed the evidence for applied research in Canada. Rea's "best guess" estimates [p. 28] reflect the very limited amount of empirical research in Canada to that time, while Osberg's "working assumptions" [p. 51, Table 3-3] rely on Canadian research to a much greater extent. Both authors concentrate on the estimates from cross-sectional data, although Rea does review the experimental evidence briefly. Both adopt an estimate of 0.1 for the compensated wage elasticity and of 0.2 for the income elasticity for men. This agreement reflects both the narrow range of early estimates for men and the lack of empirical research on this question in the 1980s, particularly in Canada. For married women, Rea suggests a compensated wage elasticity of 0.75 and an income elasticity of 0.25, while Osberg proposes 0.1 and 0.3. Their disagreement again reflects the state of the literature. Female labour supply has been studied extensively in the 1980s in both the United States and Canada, but the range of estimates remains large and, except for a few studies to be reviewed below, stubbornly unreconciled.

The lack of consensus for labour supply estimates, particularly in the case of married women, calls for continued research to reconcile previous results. In this chapter, we summarize the state of our knowledge about the crucial labour supply parameters by examining the evidence in Chapters 2 and 3 and also by considering some attempts to reconcile the evidence for married women. We shall consider both cross-sectional and panel data.

A Review of the Evidence

In Charts 4-1 and 4-2, we plot the estimated compensated wage and income elasticities over time (by year of publication) for men and women. A later publication date roughly reflects a more sophisticated econometric treatment of the data, in particular techniques that deal with non-workers and sample selection bias and models that consider nonlinear budget constraints and endogenous tax rates. A later publication date also reflects better data in some cases, in particular panel data that correct for unobserved heterogeneity and experimental data that introduce exogenous wage and income variation.

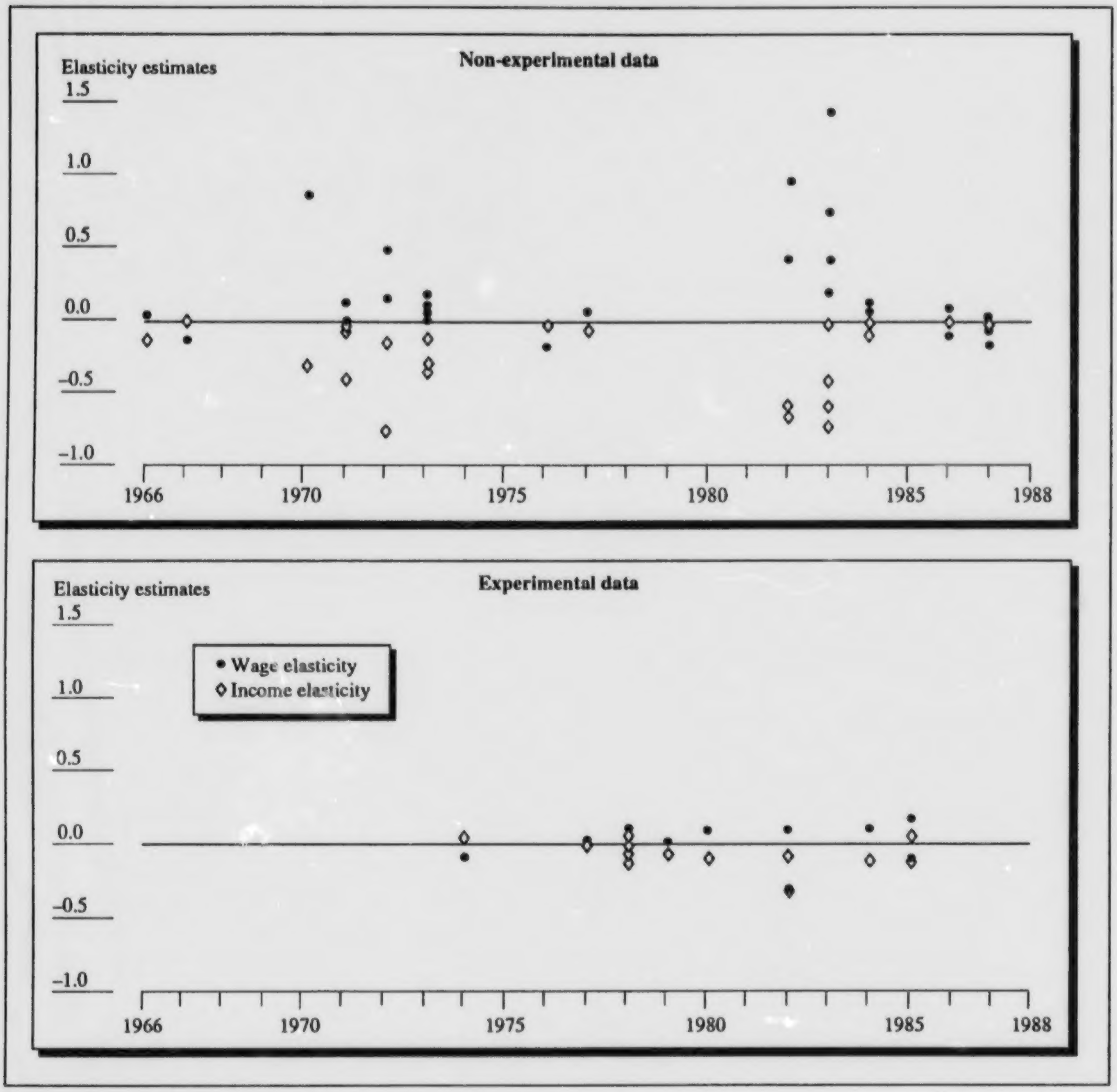
From Chart 4-1 there is very little to say concerning elasticity estimates for males because of recent research inactivity. Estimates largely remain those of an earlier time and a less sophisticated methodology, reflecting the conventional view that the more interesting economic and econometric issues concern female labour supply. The limited amount of recent evidence, including the experimental findings, generally supports the early results of very small, almost exactly offsetting, compensated wage and income elasticities. Moreover, the evidence on the uncompensated cross-wage elasticity suggests that it is negligible. Hence adult male labour supply appears to be unresponsive to changes in household wages and income.

For women, Chart 4-2 suggests that there remains a disturbingly large variation in estimates with the passage of time. Most of the estimates refer only to married women because of the view that marital status matters more for women than for men and that it is married women whose behaviour differs most from men. Yet there are very large estimates of the compensated wage and income elasticities for single women as well [e.g., Prescott et al., 1986]. Although the range of estimates is smaller for studies using experimental data, they are still much larger than those for males. Moreover, the estimates of the uncompensated cross-wage elasticity for married women (not shown; see Tables 2-3 and 3-4) cover a very wide range of negative values as well.

Mroz [1987] provides some reconciliation of the different approaches to the estimation of the labour supply parameters

Chart 4-1

Estimates of the Compensated Wage and Income Elasticities for Men, Based on Non-Experimental and Experimental Data, by Year of Publication

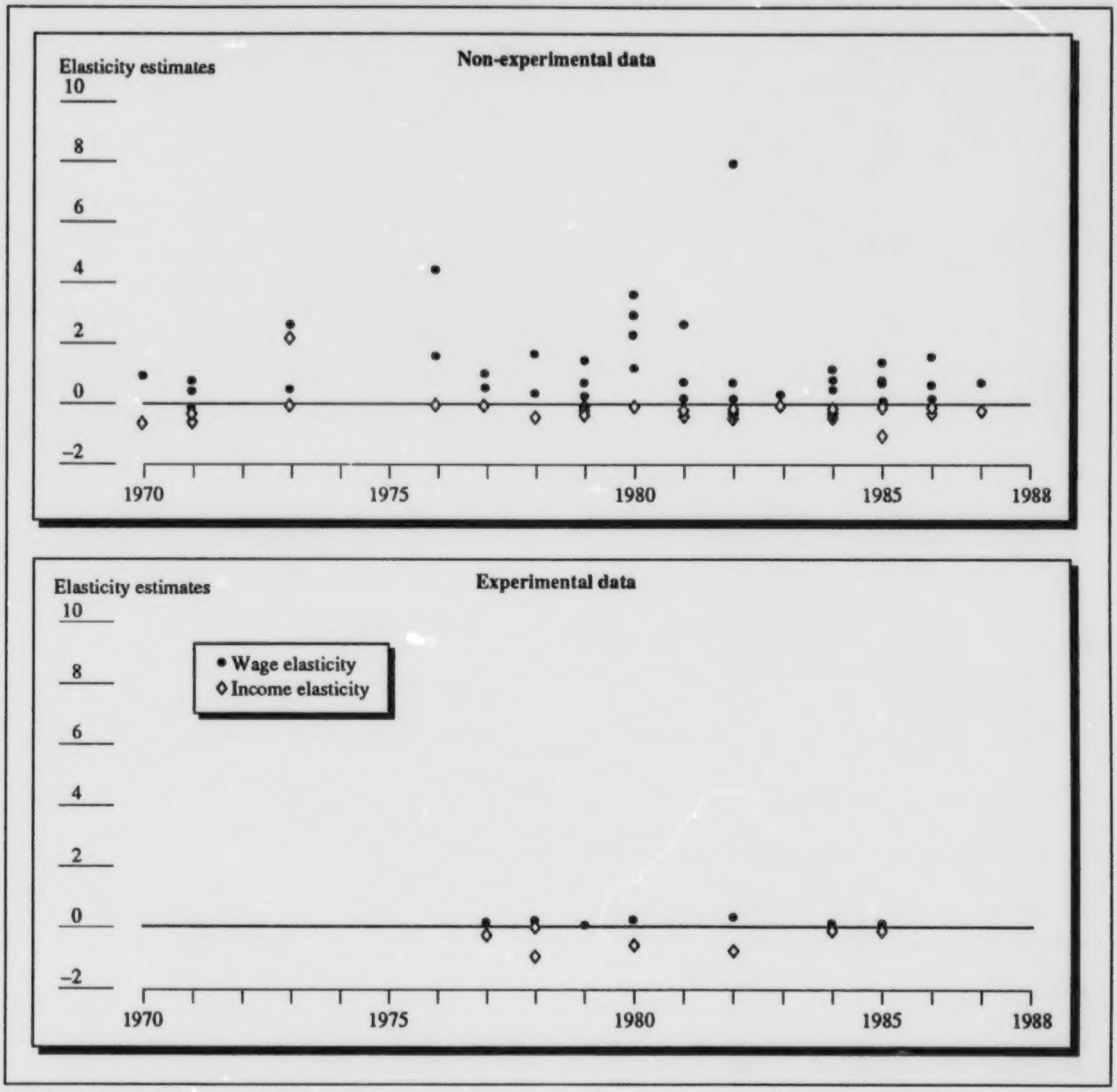


SOURCE Tables 2-1, 3-1, and 3-5.

for married women. He uses a single data set – the Panel Study of Income Dynamics, 1975 – to investigate a variety of economic and statistical assumptions for a simple linear model of individual labour supply. He finds two issues to be crucial to the estimates obtained: the treatment of the wage rate and the treatment of sample selection bias. His tests reject both the assumption that the wife's wage rate is exogenous and the assumption that the wife's labour market experience can be used as an exogenous instrumental variable to correct the bias arising from the endogeneity of the wife's wage. Past labour market experience is correlated with unobserved person-specific differences (heterogeneity)

Chart 4-2

Estimates of the Compensated Wage and Income Elasticities for Women, Based on Non-Experimental and Experimental Data, by Year of Publication



Source Tables 2-2, 3-2, 3-3, and 3-6.

that yields a spurious positive correlation between wages and labour supply in cross-sectional studies. The assumption of exogeneity for other important control variables – particularly non-wife income and children – cannot be rejected, however. Mroz also finds that the Tobit assumption to control for bias arising from self-selection into the labour force is also not supported by the data and exaggerates both wage and income effects.

Mroz's findings imply that many previous estimates of the wage and income elasticities for married women are biased upward. If, then, we were to rule out previous studies that do not treat the wife's wage as endogenous, and that use labour market experience as an instrumental variable to control for endogeneity of the wage rate, or that use the Tobit assumption to correct for selection bias, we are left with only studies with low elasticity estimates, including Boskin [1973] and Nakamura and Nakamura [1981]. Mroz's own results for acceptable specifications also are smaller in size and range than earlier results would suggest – from 0.23 to 0.85 for the uncompensated wage elasticity, and from 0.03 to 0.11 for the income elasticity. He concludes that "the range of estimates from previous studies ... is quite misleading" [p. 791] because of inappropriate econometric assumptions and that the remaining range of estimates "matches the estimated behaviour of prime aged males" [p. 795].

Mroz uses a linearized budget constraint to model the non-linear budget constraint and endogenous marginal tax rates. He does not use the complete-budget-constraint approach of Hausman [1981]. Consequently, Mroz is not able to rule out the possibility that it is Hausman's more complete treatment of taxes that accounts for his large elasticity estimates. His tentative conclusion, however, is that "the influence of taxes ... appears to be at most a second order effect" [p. 786].

Jakubson [1988] provides a further study of the labour supply of married women under alternative econometric assumptions. His focus is on the difference in the estimates between cross-sectional data and panel data, since the latter permit him to correct for the unobserved person-specific differences across individuals (see Chapter 3). He uses the 1968, 1970, 1972, and 1974 panels of the Panel Study of Income Dynamics to estimate three models: a cross-sectional Tobit model on individual panels; a random-effects model on all the panel data; and a fixed-effects model on all the panel data. His major conclusion is that "cross-section estimates of female labor supply are biased away from zero because of the omission of an individual specific component that is correlated with observed children variables" [p. 318]. His models do not permit estimation of wage and income elasticities.

The results from Mroz and Jakubson agree in several respects. Both focus on unobserved person-specific differences as the source of bias in cross-sectional data, Mroz through the wage rate and Jakubson through children. Both find that the biases are away from zero (larger elasticity estimates). On the other hand, Mroz does not find evidence that children are endogenous, as Jakubson's results suggest, and Jakubson's use of the Tobit model (and, it would appear, labour market experience in the wage equation) is unfortunate in view of Mroz's findings. The Tobit model could account for the bias in Jakubson's study according to Mroz's results. Clearly, more research of this nature using a variety of econometric models and assumptions on a single data set is needed to reconcile further the range of estimates portrayed in Chart 4-2. Comparable work for men would also be a useful check on the consensus derived from early, less sophisticated econometric methods and data.

The Role of Experimental Data

It would appear that the range of elasticity estimates from experimental panel data is smaller, and at the lower end of the estimates, compared to those obtained from cross-sectional data. If estimates from cross-sectional data tend to be biased upwards, as Mroz [1987] and Jakubson [1988] suggest, then the estimates from experimental data are a more reliable guide for economic and social policy analysis. There may be cross-sectional studies that are reliable, but it is easy to adopt econometric assumptions that lead to large, positive biases in the elasticity estimates. Such biases are not apparent in studies using experimental data, despite a variety of approaches to estimation.

The reliability of parameter estimates from experimental data may arise from several sources. First, the wage and income variations are primarily experimentally induced and hence essentially exogenous and unanticipated. Endogeneity of the wage rate, found to be an important problem by Mroz, should not be a serious problem here. Second, the panel data allow the researcher to correct for unobserved person-specific differences by examining changes in individual behaviour before and after the experimental treatment. Both Mroz and Jakubson cite control for person-specific differences as a major source of bias in cross-sectional studies. Third, experimental treatment simplifies the tax structure, allowing the researcher to avoid complex models that correct for non-linear budgets and endogenous tax rates.

The experimental data sets currently available offer no panacea, however. Despite their promise, they have their own unique set of problems, including income truncation, non-random assignment and small sample size, and attrition. All of these problems must be explored further. Still, experimental data offer a valuable source of information on labour supply behaviour that should not be ignored. Mroz [1987, pp. 794-95], in reflecting upon his attempt to sort out the various elasticity estimates for married women, concludes as follows:

> The negative income tax experiments provide additional data sources for cross-validating the income and substitution effects found in this study. It has been argued that these experiments do permit one to examine more "exogenous"

> variations in wages and incomes than can be found in nonexperimental data. Indeed the range of estimates reported by Moffitt and Kehrer [1981] for the estimates of the wage and income effects for married women from the experimental literature suggests smaller responses than one would infer from a casual examination of the estimates. . . . Given the conceptual difficulties of analyzing the nonrandom treatment and control assignments in the experimental data and the small number of studies directly estimating behavioral responses, we are hesitant to use the experimental results as a strong cross-validation of our estimates. However, we do believe that a thorough examination of the experimental data could provide a strong test of the robustness of the results found in this study.

The conclusions of Mroz may seem disappointing at first. Nonetheless they must be taken to heart, especially since we have noted that Canadian empirical research on labour supply behaviour is so sparse. At the same time, Canadian experimental data provide another opportunity to investigate labour supply behaviour using this rare and, in many respects, advantageous type of data. This is the subject of Part B of this study.

Part B

The Canadian Mincome Experiment: Background and Labour Supply Evidence

5 Some Background to the Mincome Experiment

This chapter describes the background to the Mincome experiment. It sketches the circumstances leading to the federal-provincial review of social security and the Mincome experiment, and how the experiment evolved from idea through implementation to final disposition. The review of social security was a highly publicized evaluation of Canada's system of social programs, aimed at achieving new federal legislation for cost-sharing. However, the review lapsed without the hoped-for revisions to the Canada Assistance Plan or any other jointly agreed basis for cost-sharing social assistance. In the meantime, the parallel Mincome project also proved disappointing; it produced no final report and no research on the question of work incentives. Indeed, few Canadians are even aware that Mincome took place at all or that a wealth of data remains largely unexplored by researchers. For these reasons, it seems appropriate to record the events surrounding the Mincome experiment. Those mainly interested in empirical results may omit this chapter, but others with an interest in policy development should find our discussion informative.

Prelude to the Review and Experiment

The Canada Assistance Plan came into effect in 1967 and was to be the centrepiece of Canada's antipoverty efforts.[1] At almost the same time, poverty in Canada received "official" recognition when the Economic Council of Canada released its Fifth Annual Review in 1968. Two years later, a report by the Department of National Health and Welfare, *Income Security for Canadians* [1970], proposed to reform family allowances by introducing the Family Income Security Plan – an income-tested program. A Senate Committee report, *Poverty in Canada* [1971], called for a national guaranteed annual income based on the negative income tax principle, that is, delivered on an income-tested or selective basis. The Quebec Commission of Inquiry on Health and Social Welfare [the Castonguay-Nepveu Commission, 1971] recommended major restructuring and integration of social programs in Quebec. Changes to the Unemployment Insurance Program were also put into effect in 1971. These represented a substantial liberalizing of regulations and enhancement of benefits and were made without provincial consultation. The federal government appeared willing to initiate reform and to incur additional social spending. There was active discussion of policy reform options, curiosity concerning the potential of a guaranteed income, and emerging suspicion that the provisions of the Canada Assistance Plan would eventually have to be amended or replaced. Economic restraint was not the order of the day. Nevertheless, the impetus for social security reform came from another quarter – federal-provincial relations and the Constitution.

In 1971, a federal-provincial conference was held in Victoria in an attempt to rewrite and "patriate" the Canadian Constitution. The provinces and Canada appeared to reach agreement when Quebec declared that it could not support the "Victoria Charter" because, in part, it "failed to provide for a jurisdictional settlement in the field of social policy" and "no patriation of the Constitution would be possible until those concerns were satisfied" [Van Loon, 1979, p. 474]. There was much discontent in the federal-provincial relations after the Victoria conference, and this surfaced in 1972 at the Conference of Provincial Welfare Ministers. Federal disappointment over the failure to patriate the Constitution (including an amending formula) was deep. Provincial dissatisfaction was fueled by the federal government's unilateral changes to unemployment insurance in 1971 and its proposed reform of family allowances.[2] Indeed, several provinces demanded at the conference that the federal government turn over to them the jurisdiction and fiscal resources for family allowances [Johnson, 1975, p. 475]. There was resentment over federal intrusion into provincial jurisdiction with what provinces felt were ill-conceived and uncoordinated programs. Provincial frustration "had reached an all time high" [Doyle, 1978, p. 26]. Thus when the Conference of Provincial Welfare Ministers unanimously called for a joint review "to develop better mechanisms for achieving a rationalized social security system in Canada," the federal government quickly and readily agreed [Johnson, 1975, p. 457].

There were political pressures on the federal government at the time as well. The Liberals were in a minority government position with the New Democratic Party (NDP) holding the balance of power. The minority Liberals had to agree informally to reconsider social programs in return for the support of the NDP in Parliament. Furthermore, of all three national parties, it was the NDP that expressed most publicly its commitment to the idea of a guaranteed income.

The Throne Speech on January 4, 1973, opening the twenty-ninth Parliament called for a federal-provincial review of the nation's social security system. This was quickly followed in April 1973 by the federal *Working Paper on Social Security in Canada* (the Orange Paper), a document that set the stage for the subsequent discussions. Coinciding with this interest in reviewing social security, policymakers began considering the advantages and disadvantages of the guaranteed income concept. Early attention naturally focused on the possible work disincentive effects of a guaranteed income. And note was made of the income maintenance experiments being conducted in the United States. The White Paper, *Income Security for Canadians* [1970, p. 41], proclaimed:

> An overall guaranteed income program for the whole population that is worthy of consideration is one that offers a substantial level of benefit to people who are normally in the labour market. Therefore, a great deal of further study and investigation, like the experiments now under way in New Jersey and Seattle in the United States, is needed to find out what effects such a program would have on people's motivation, on their incentives to work and save. Until these questions are answered, the fear of its impact on productivity will be the main deterrent to the introduction of a general overall guaranteed income plan.

During 1971, Manitoba showed serious interest in testing the guaranteed income approach, particularly as a demonstration project or administrative test. Edward R. Schreyer, then Premier of Manitoba, declared on September 9, 1971, Manitoba's commitment to an experimental pilot project on the guaranteed annual income concept. In March 1973, Manitoba submitted a proposal for a guaranteed income project for funding to the Department of National Health and Welfare. The proposal was approved two months later, and on June 4, 1974, Canada and Manitoba formally signed an Agreement Concerning a Basic Annual Income Experiment Project covering cost-sharing arrangements and the respective roles of the two governments. The Manitoba submission coincided with the release of the Orange Paper and the start of the joint federal-provincial review of social security.

The Manitoba Basic Annual Income Experiment (Mincome) sought to evaluate in rigorous manner a guaranteed income program within the broader context of an overall review of the social security system. As such, it responded to the generally recognized need to reform the income security system and to the increasing interest in the concept of guaranteed income. The social security review and the guaranteed income experiment were therefore connected in thought and purpose in addition to timing. The review is sometimes regarded as an attempt to develop an integrated and coordinated approach to social security, or to supplant certain portions of the Canada Assistance Plan legislation [federal and provincial Ministers of Welfare, communiqué, 30 April 1975], or even as a surrogate for the constitutional discussions adjourned at Victoria [Van Loon, 1979]. But the National Council of Welfare [1976] did not fail to see the connection; it bluntly asserted that "the goal of the social security review [was] the establishment of a guaranteed annual income." Similarly, the Mincome project was more than just an expensive exercise in econometrics or a brave new adventure in social experimentation. The joint news release [22 February 1974, p. 5] announcing the final approval of the experiment by Canada and Manitoba was quite clear about the role and purpose of the guaranteed income test. It proclaimed: "The Manitoba experiment is expected to make an important contribution to the review of Canada's social security system launched last April by all ten provinces and the federal government."

The Guaranteed Income Experiment

The design of Mincome focused on the question of work disincentives, but there was also strong interest in administrative and operational issues. Indeed, this was the basis for its original support by Manitoba. On June 7, 1971, the Minister of Health and Social Development for Manitoba, R. Toupin, declared at a Conference of Federal-Provincial Ministers of Welfare that Manitoba was convinced of the validity and justness of the guaranteed annual income approach. The Minister said it was "of vital interest to examine the complexities involved in the administration of guaranteed income programs" and that "much knowledge [could be] gained . . . in a demonstration project." Manitoba saw the "proposed demonstration study project as a Federal-Provincial responsibility" and requested cost sharing under the Canada Assistance Plan. The Minister's statement concluded by asserting that "Manitoba is prepared to conduct the demonstration without federal participation if it cannot be obtained within a reasonable period of time."

Three months later, Premier Edward Schreyer reiterated this position in a major speech. However, details of his thinking can best be gauged through a lengthy interview published by the *Winnipeg Tribune* [10 July 1971]. Premier Schreyer viewed the guaranteed annual income as essentially involving "income-testing" and that it "didn't differ at all [from] a negative income tax." Furthermore, because the guaranteed annual income would "substitute for the

Canada Assistance Plan Program," the Mincome "project would be established under the aegis of the Canada Assistance Plan." He saw Manitoba's financial involvement at "something over $500,000" and the number of families involved "possibly 500" but "closer to 300." Shortly after, Canada announced a program to cover 75 per cent of the costs of such projects. Manitoba submitted a detailed proposal to Canada in March 1973, but it was an outline for a truly scientific research experiment; it was to cost in excess of $17 million, and it contemplated enrolment of well over 1,000 families.[3]

At this point, a clear distinction must be drawn between "demonstrations" or "pilot projects" and "social experiments." The notion of an experiment is exemplified by the research approach commonly employed in the natural or physical sciences. Generally speaking, the procedures entail special actions to support or falsify hypotheses under controlled conditions, where control is effected by the experimenter either by actually fixing certain conditions or statistically by randomization.[4] The term "experiment" therefore strictly excludes "demonstrations" and "pilot projects." Demonstrations are often employed to dramatize some program already selected on a priori considerations as the committed course to follow, while pilot projects are typically feasibility studies of some proposed mechanism to test procedures or detect unforeseen features.[5]

How, then, did the Mincome project evolve from a demonstration to a true experiment? There was tremendous interest among federal officials in the U.S. projects, which were designed as research experiments. The idea of a similar endeavour in Canada was tantalizing. Furthermore, given that Manitoba was already committed to a demonstration and sought federal funding, it seemed natural to combine in one single undertaking experimental research and administrative testing. The relevance of the guaranteed income concept to both the social security review and the Canada Assistance Plan made what seemed logical and useful also opportune. Accordingly, what emerged was not the simple demonstration involving 300 families and $500 thousand that Manitoba wanted, but an extremely complicated scientific experiment, modelled along the lines of the pioneering U.S. efforts and concentrating on the issue of work responses.

Mincome's design involved selecting participants randomly and assigning them to alternative negative income tax programs for a three-year period. Families were selected from three sites: the city of Winnipeg, the community of Dauphin, and a number of small rural communities. Since the primary research objective was work response, the experiment excluded the aged, the disabled, and the institutionalized from participation.[6]

The unique feature of the Mincome design was its "saturation" site: every resident in Dauphin was eligible to participate in a guaranteed annual income program. The Winnipeg portion of the sample was a randomly drawn dispersed sample. Its methodological advantage was the ability to isolate treatment families from one another, thereby making it possible to experimentally vary the parameters of the negative income tax program within the same area. However, this isolation placed treatment families in a highly unrealistic environment – quite unlike the circumstances that would exist under a national negative income tax program whereby all eligible families could receive cash transfers. Mincome therefore included the saturation site (Dauphin) in the hope of answering questions about administrative and community issues resulting from a less artificial environment. The final design of Mincome may be described then as a hybrid. It conformed to the format of the "classic" experiment (the Winnipeg portion), but it also included "demonstration" aspects (Dauphin).

The organization and conduct of the experiment deserve to be noted. The early design work had been planned jointly by federal and provincial personnel. Once under way, it was announced that while research would be undertaken jointly, the province would be solely responsible for operations. Since Mincome was ostensibly a research project, this amounted to joint control of all important decisions. There was much to be said for this structure. The joint effort would set a precedent for future endeavours of this type, both governments would receive any credit due, both bureaucracies would become equally committed to the project, both governments would determine the "political" matters, and both administrations would gain expertise and experience. But there were also disadvantages. Decision making might be hesitant or slow. Deadlock between the two bureaucracies was a possibility, and opportunities for political interference would be maximized since both governments were involved; conflicts concerning unrelated matters might show up in discussion of experimental decisions. The potential for conflict and confrontation was therefore great. But what were the options? Unilateral action? The federal government probably could not implement the experiment itself because provincial cooperation was required to deliver the "treatment" program. Despite Manitoba's bravado about "going it alone" on a demonstration project, a truly scientific experiment of the magnitude actually proposed was too expensive. For either government to attempt the project on the scale proposed in 1973 by itself was consequently too risky. One possibility might have been to establish an independent Crown corporation to conduct the experiment. Although this might assure the research integrity of the project, a Crown corporation could also prove cumbersome.

Further, it would cost much more because certain overhead expenses associated with activities such as delivering cheques and the like would no longer be available through line departments. A new independent Crown corporation might also have difficulty establishing credibility, but, more importantly, social experimentation is the kind of research that necessitates direct government involvement at times. Bearing in mind the cooperative spirit between Canada and Manitoba at the time, joint control of the experiment seemed, therefore, the right and proper choice.

Despite auspicious beginnings, dual control of the experiment created many difficulties. Not the least of the problems encountered was distinguishing between research issues, which were to be resolved jointly, and operational concerns, which were a provincial responsibility. More than midway through the experiment, conflict between Manitoba and Canada led to a tentative consideration of alternative arrangements, at least unofficially, among department officials. The "Crown corporation" or "independent institute" solution was proposed, but nothing came of these discussions. All parties apparently knew too well that a new agreement would have been necessary, and that much disruption to the experiment would result. Furthermore, the social security review was dissipating and, though unsaid, the experiment's senior people knew that interest and political support for the guaranteed income concept was waning. The question of an effective organizational structure for Mincome was never satisfactorily or directly resolved.

The actual conduct of the experiment, of course, had its ups and downs. There were scientific difficulties, political difficulties, management difficulties, operational difficulties – the list goes on. It is not particularly instructive to document all these details. What is significant was the change in "mandate" of the experiment midway through its course. Instead of the implied sequence of design, experimental trial, data collection, analysis, and the production of research results, perhaps culminating in an official report, the experiment was altered in two fundamental ways. First, the research priority was shifted away from the original work disincentive issues towards administrative questions. Second, and more significantly, the Mincome project was directed to adopt an "archive" strategy. That is, it would collect data but not analyse them.

There were a number of reasons – all interrelated – for this turn of events. The most fundamental and basic was a lack of funds. The experiment had been designed with a total budget of slightly over $17 million in mind. No one really believed that this figure was anything but a wild guess. The number was a convenient one; it had the right "feel" in terms of getting the project approved. As the experiment proceeded, more accurate estimates would be possible and, accordingly, budgets could be redrafted to reflect actual expenditures and resubmitted. Or so it was thought!

The budget total of $17 million soon solidified into a "hard" number. It became the contractual maximum for both governments for a variety of reasons and the problem then became one of spending this total in the best manner possible. At first, the usual cost-cutting measures were taken, including eliminating entire research programs thought to be secondary.

The experiment originally contemplated extensive research programs in four major fields: economic topics such as the work disincentive question, job search, wage impacts, and the like; sociological topics such as marital stability, geographical mobility, and so on; administrative issues; and, lastly, statistical questions. Of these, economic issues – particularly the work disincentive question – were given top priority. Consequently, the first research programs to be eliminated were the sociological topics and farm labour supply projects. The sociological subjects had no real base of support or champions among the project's permanent senior research staff. In fact, there was actual resentment towards the sociological topics, partly because much of that research was being done by "consultants," and partly because some of the methods were not in high favour among a predominantly quantitatively orientated staff. The technique in question is often termed "ethnomethodological research" and involves researchers engaging in fairly in-depth and open-ended interviews with the participants in order to gauge their attitudes, responses, reactions, and the like. Certainly, when press accounts sensationalized the program as one whereby all privacy had to be forgone in exchange for a guaranteed annual income, moral support for the sociological research program diminished further still.

The research on farm labour supply did not have widespread support either. Federal officials regarded its presence on the research agenda as a concession to Manitoba with its "farming interests." Consequently, a "small farm sample" was approved. As matters turned out, the difficulties of capturing accurate data concerning farm labour practices, the diminishing sample size due to attrition, and the extraordinary administrative demands to deliver the guaranteed income program to farm families all combined to make the research area no longer worth the effort. When financial pressures eventually made some cost cutting necessary, there were few regrets in sacrificing the research on farm labour supply. Thus, when it finally became clear that no additional money would be forthcoming and that the remaining funds would not be sufficient to conduct a credible investigation of even a scaled-down version of the work disincentive question, there was virtually no alternative but to declare the project a "success" and to close it down gracefully. The project's mandate was therefore redirected towards administrative issues for its remaining life. The project was told to prepare the data already collected for storage and to document whatever work had already been completed. The data, it was hoped, would at least be preserved for analysis at a later date.

The significance of the fixed budget amount established at the beginning of the experiment cannot be overestimated. It must be remembered that guaranteed income payments to the participating families came out of this total amount, and that such payments were adjusted annually by an indexing factor. In other words, the amount allotted for payments was not under the experiment's control; it depended upon such factors as the income of the participants, the rate of inflation in the Canadian economy, and the like. Yet these funds had first claim on the $17 million total because of their "statutory" nature. After these payments, operational costs for data collection, overhead, survey personnel, and so on would have the second claim on funds simply because these bills came next in the normal sequence of activities. Research constituted the last stage of the process and therefore would receive the residual of funds. Given the fixed budget amount and the above expenditure sequence, it is not at all surprising that the experiment was "truncated." With both time and money running out, an archival strategy seemed best for the long run. In the short run, it was also convenient for the experiment to address operational questions with its remaining budget. Administrative questions would be relatively cheap to research and, unlike the work disincentive question, could not really be deferred to another generation of researchers. Besides, administrative and operational concerns were of interest in their own right.

In giving prime importance to the initial terms of the funding of the project, we do not wish to imply there were no other factors. Our aim is simply to suggest there were no villains in the piece, that the project was not shut down for purely political considerations, and that research activities were not "cut off" because any party feared their expected conclusions. The budget total is simply meant to indicate the logic behind the experiment's change in mandate and its eventual disposition. To be sure, there is a larger story to be told and obvious questions are raised, such as: Why was the budget simply not increased? In fact, there were concerted attempts to obtain funds or seek other sponsors. Staff at Mincome developed alternative research proposals to investigate labour supply, much more modest designs, and hoped that minimal additional funding would be possible. There was even talk that Manitoba would consider "full financing" of the research phase alone, but this was probably not really serious. Canada could hardly allow itself to be embarrassed by cost-sharing more than $17 million in gathering data, only to have Manitoba reap the entire research glory for a mere additional $1 or $2 million.

It must also be remembered that Mincome's redirection came towards the end of the 1970s. The social security review had ended; there was no political support in the country for sweeping reforms of the type promised by a guaranteed income; and the guaranteed annual income concept itself had lost its fashionable patina. Furthermore, the economy's performance was extremely poor and a program of restraint was announced; there had also been a change in government both in Ottawa (with the election of Joe Clark) and in Manitoba (with the election of Sterling Lyon). Given all this, it is understandable why everyone wanted Mincome to conclude quietly and gracefully. The experiment officially ended in 1979.

During the next two years, the fate of the data themselves appeared uncertain. The signs were very discouraging. Indeed, the manner in which the data were archived (unpublicized location, unknown means of access, and so on) was not publicized among the research community. In 1981, the Institute for Social and Economic Research was created at the University of Manitoba with funding from the Department of National Health and Welfare. One of the major aims of the Institute was to prepare the Mincome data for analysis by researchers.[7]

There was also a general expectation that the Department of National Health and Welfare would, from time to time, fund analytic studies on specific topics of income maintenance, poverty, and social welfare. But this never materialized, perhaps partly because of disinterest in the potential of the Mincome data, perhaps partly because long-range research in work disincentives is not part of the Department's normal process or mandate, but also perhaps because the necessary support and knowledge to execute the experiment's original objectives do not exist within the Department. For quite some time, discussion in Canada concerning the effect of a guaranteed income on work behaviour had to rely primarily on U.S. research.

6 An Overview of the Mincome Design

The participants in the Mincome experiment were selected randomly from a number of sites and assigned either to one of several negative income tax treatment programs or, alternatively, to a control group. The participants assigned to the treatment programs were given monthly income-conditioned payments for three years. Those in the control group were not entitled to a guaranteed income. All the participants were interviewed three times a year to obtain detailed information on their work effort.

This chapter presents an overview of the design of Mincome. Full details are available in technical reports documenting the experiment. Furthermore, the discussion in this chapter is general; technical details, where necessary, are included in the following chapters that analyse portions of the data themselves.

Objectives of the Experiment

The Mincome experiment was concerned with the evaluation of a negative income tax program. Such a program, if introduced on a permanent and universal basis, would have a wide range of effects. Nonetheless, the research focus of the experiment was the labour supply response of families and individuals to various guaranteed annual income programs. Labour supply included, for example, such topics as the labour force participation decision of primary and secondary earners; the number of hours worked; the job search process, including time and resources devoted to job search; job satisfaction and attitudes towards work; and issues pertaining to investment in human capital.

Labour supply became the primary concern because it has two types of cost implications if a permanent negative income tax program were established. First, changes in labour supply will result in changes in family income and this, in turn, will affect the cost of transfer payments. Second, systematic changes in individual work pattern for large numbers of the population can affect the total labour supply available and result in lower national income. At the same time, changes in the supply of particular types of labour could cause severe adjustment problems for industries, sectors, or regions dependent upon that type of labour.

In addition to labour supply questions there were a number of secondary research objectives. Some of these include: 1) family behaviour, especially family formation and disintegration; 2) youth behaviour, and whether or not their response is different from the general population; 3) community involvement and participation, including cooperative behaviour and community-related values and attitudes; and 4) geographic mobility, whether or not a guaranteed income will affect the decision to move.

There are a number of limited objectives in the area of program administration. These include the assessment of: 1) the accuracy of the information supplied to the program by the participants; 2) the participants' comprehension of the administrative structures; and 3) the cost, responsiveness, and equity of the experimental delivery system.

Description of the Payment Formula

A negative income tax program may be characterized as a universal objective, income-conditioned transfer mechanism aimed at minimizing work disincentive. The program is universal since it is not limited to specific recipient groups; it is objective since entitlement is not subject to administrative discretion; it is income-conditioned since the major eligibility criterion is the income of the recipient; and, finally, it encourages labour force participation by reducing the benefit entitlement by less than the full amount of any earned wages, hence the individual is always left with a higher net income from working than not working. The negative income tax system of Mincome can be described in terms of two basic parameters: an annual support level and an offset tax rate. The annual support level (G) depends upon both family size and composition and represents the amount (guaranteed annual income) the family receives if it has no other income or wealth. As the family's income and/or net worth increases, the payment declines. The rate at which increments in earned income are offset against payments is determined by a constant offset tax rate (t). The rate at which increments in wealth are offset against payments is referred to as the net worth tax rate (r). The simplest form of this program for a typical family unit may be algebraically represented by:

$$P = G - t \cdot Y - r \cdot W,$$

where

P = the payment under the negative income tax program;
G = the support level for a representative family;
t = the offset tax rate;
Y = family income;
r = the net worth tax rate; and
W = family net worth (assets minus liabilities).

The participants receive negative income tax payments as long as their income falls below a certain "break-even" level, defined as G/t (ignoring wealth).

The Experimental Treatments

The research objective is to predict work behaviour in response to guaranteed income programs that differ only in terms of support levels and negative tax rates. The total cost of any income maintenance program will depend in large part on the actual support level and particular negative tax rate adopted. Accordingly, the program characteristics of most interest are the guaranteed support level (G) and the tax rate (t).

Three different support levels and three different offset tax rates were used in the experiment in order to allow detection of non-linearities in response. Omitting the combination of the highest guarantee level with the lowest tax rate yields eight possible financial treatment plans. However, the addition of a control group results in a total of nine plans to the design.

Integration with Other Tax-Transfer Programs

The guarantee support level and the offset tax rate are the only program parameters that vary directly; other basic parameters are maintained constant over all financial treatment plans. To ensure that participants in the experiment are in fact facing the support levels and tax rates designated by their financial treatment, the multitude of other government tax and transfer programs must be taken into account and neutralized. The basic design task is one of maintaining control over the marginal tax rate faced by participants and the level of other government transfers. This is important since determining the response of individuals subject to more than one tax rate as a result of simultaneous participation in several income-conditioned programs with differing eligibility conditions is extremely difficult. Accordingly, all other government transfers received by a participant in the experiment are subject to a 100-per-cent tax and all other income-conditioned taxes paid by participants are subject to complete rebate, thereby integrating the negative income tax program with existing tax-transfer systems while simultaneously ensuring that participants face the support level and the tax rate of their financial treatment.

Features of the Payment System

The support level is guaranteed to the family over a 12-month period. Payments, however, are made on a monthly basis. Families report total income from all sources monthly and are paid a sum each month based upon reported total income in the previous month plus an adjustment amount. The adjustment occurs because reported income above the break-even level is "carried forward" and counted as income in months when income falls below the break-even level. However, the accounting period, that is, the time interval over which income is measured for the purposes of determining payments, is one calendar year. Consequently, actual monthly payments received by the family over the accounting period are reconciled with their entitlement based upon the total income received within the same period. Underpayments to families are corrected and overpayments are recovered.

Population and Sample Structure

The group of interest for the experiment is much more narrow than the population eventually covered by a universal guaranteed annual income program. Given the research focus on work effort, the aged, the institutionalized, the disabled, and others are excluded on the grounds that the experimental treatments were unlikely to produce any relevant work behaviour response.

Similarly, the research objectives guide the classification and stratification of the sample. The categories extend over a variety of family types and a range of normal incomes. The sample distinguished the following: 1) double-headed families with both heads working; 2) double-headed families with only one head working; 3) single-headed families; and 4) single (unattached) individuals. The resulting stratifications reflect the hypotheses that unattached individuals will react differently than individuals with family responsibilities; that intact families will behave differently than single-headed families; and that significantly different labour supply response will be observed between secondary workers who were working prior to the experiment and those who were not. In addition, because it was expected that families with different income levels will respond differently to the experimental treatments, families were also stratified in terms of normal income, that is, an income measure from which estimated transitory components were removed and adjusted to a family size of four, comprising two heads and two children under 15 years of age. The normal income measure was based upon pre-experimental information on earnings and work history and represented an estimate of the annual income the family would normally receive.

The eligible population was also confined to residents of Manitoba and the sample drawn from: 1) the city of Winnipeg; 2) several smaller rural communities; and 3) Dauphin, where all families and individuals are eligible for enrolment in the program. The samples drawn from Winnipeg and the several smaller communities are referred to as the urban dispersed and rural dispersed samples, respectively, in that the sample size is small relative to the communities from which they are drawn. Sample points from Dauphin are termed saturation site observations.

Special Experimental Design Issues

The Mincome program limited payments to participants to three years. This may pose a problem for the prediction of effects of permanent programs because it implicitly assumes that the experimental families will make the same adjustment within three years that they would to an indefinite length national program.

For many families the process of regularly reporting their income or responding to periodic interviews may have an impact on their behaviour in addition to the effect of receiving payments. The reporting requirement may lead families to have more accurate views of their income and financial situation than they would otherwise. For policy purposes, it is important to be able to distinguish between these two responses. Accordingly, a portion of the control group also provide monthly reports of their income, thereby making the act of reporting itself a variable.

Randomly drawn dispersed samples can be used to determine individual responses that are not confounded by macro or community effects. In addition, the effect of dispersion is to isolate treatment families so that the parameters of the negative income tax program can be experimentally varied within the dispersed site. At the same time, this very isolation may place treatment families in a highly artificial environment. In recognition of this, Mincome included the saturation site wherein all members of the community are eligible for payments. This single saturation site should provide a more accurate approximation to a real-life situation, provide valuable administrative and operational experience, and possibly generate information on community effects. Among its disadvantages is the impossibility of multiple financial treatments since any horizontal inequity introduced into a single saturated community would quickly become obvious to the participants. As well, a single saturation site precludes a pure control group for comparison. Notwithstanding methodological problems, the saturation site provides, at a minimum, the possibility for administrative experience.

Sample Design and Allocation

In designing the Mincome experiment, a major problem is how to maximize precision in estimating labour supply effects given limited resources (financial and so on) while also observing additional constraints. A formal sample assignment model allows a design that optimizes the allocation of observation points among different experimental cells given that observations placed in different cells yield different benefits with respect to some objective function and represent commitments to incur different levels of costs. The assignment model employed by Mincome comprises the following components: 1) a basic regression model; 2) a set of admissible regressor rows; 3) an objective function; and 4) a set of constraints.

Mincome used an adaptation of the assignment model developed by Conlisk and Watts [1969] to achieve a sample design. The Conlisk-Watts model is a formal technique for allocating sample points among experimental treatments in order to maximize the value of the information generated by the experiment. The model conducts a benefit-cost analysis of alternative sample allocations that are feasible within a given budget constraint. In our application, benefits are measured in terms of reductions in the variances of certain predicted values. Costs are measured in monetary terms reflecting the financial budget constraint.

More formally the design model may be stated in the following manner:

$$\text{Minimize } F(n_1, n_2, \ldots, n_m) = tr\,[W \operatorname{var}(Pb)],$$

subject to

$$c_i n_i < C \qquad (L[n_1, \ldots, n_m] = 0),$$

$$n_i > 0 \qquad (i = 1, \ldots, m),$$

where

m = the number of design points;
c_i = the cost of one observation at the i^{th} design point;
n_i = the number of observations at the i^{th} design point; and
C = the total available budget.

$L(\ldots) = 0$ represents additional external constraints. The criterion to rank alternative feasible designs is given by the objective function $F(\cdot)$. An estimation viewpoint for the experiment is adopted in specifying $F(\cdot)$ in terms of estimation error var (Pb). In particular, a response function and regression model are specified and the optimal sample allocation is that which minimizes the weighted sum of the variances of the elements of Pb. Pb is a linear combination of the estimated vector of regression coefficients b, $tr(\cdot)$ is the trace operator, and W is a positive definite diagonal weight matrix whose diagonal elements measure the relative importance (policy relevance) to the experiment of the elements of Pb.

The above is a well-behaved programming problem involving minimization of a convex objective function over a set of linear constraints. Accordingly, the assignment model indicates for a given budget the optimal allocation of sample points that will yield the least prediction error for an estimated regression equation.

With the above general description of the design and sample structure of the Mincome experiment, the next chapters analyse the urban dispersed data, this being the portion representing the classical experimental design.

7 Analysis of the Experimental Effect on Labour Supply in Mincome

We argue in Chapters 3 and 4 that experimental panel data can provide superior evidence concerning the effects of the tax and transfer programs on labour supply behaviour. In the remaining chapters we illustrate this argument using the Mincome database for intact Winnipeg families.[1] In this chapter we discuss the statistical analysis of panel data as it applies to experimental data. In Chapter 8 we consider the strengths and weaknesses of alternative statistical models and methods of estimation. We then turn to the analysis of the Mincome data using a variety of these models and estimation techniques to try to reconcile the empirical results, both experimental and non-experimental, surveyed in Part A. Our summary in Chapter 9 incorporates the findings from the Mincome data into our assessment of the state of empirical knowledge about labour supply response.

Models and Analysis for Panel Data

Panel data involve a series of T cross-sections with N individuals in each cross-section. The data include a dependent variable,

$$Y_{it} \qquad (i = 1, \ldots, N;\ t = 1, \ldots, T),$$

that varies over time and across individuals. In the case of Mincome, Y_{it} will be individual annual hours worked as our preferred measure of labour supply. The data also include a set of independent variables represented by a row vector,

$$X_{it} = (X_{1it}, \ldots, X_{Kit}) \qquad (i = 1, \ldots, N;\ t = 1, \ldots, T),$$

for each individual in each time period.

Models for analysis of panel data are able to consider *unobserved* individual-specific effects, α_i, and time-specific effects, η_t, as well as the standard disturbance term, e_{it}. Thus we can write the model as:

$$Y_{it} = X_{it}\beta + \alpha_i + \eta_t + e_{it}. \tag{7.1}$$

Lack of attention to individual-specific effects, α_i, or worker heterogeneity, is often cited as an important source of estimation bias in the labour supply literature ("ambition" or "motivation" in Chapter 4; "marginal utility of wealth" in Chapter 5). There may also be significant time-specific effects, η_t, related to local economic conditions or experimental duration, to consider.

Unobserved effects may be analysed in two ways. We can analyse α_i (or η_t) as a fixed effect for each individual (time period) in the sample or we can analyse α_i (or η_t) as a random effect drawn from the population of individual effects [Hsiao, 1986, pp. 41-43]. First, consider α_i and η_t as fixed effects. Since Mincome contains a large number of individuals over a few time periods, we are interested in estimating the time effects η_t and adjusting for, but not estimating, the individual effects α_i. We can accomplish this by using the within-groups estimator. Let the mean observation for all time periods for any individual be:

$$Y_{i} = \frac{1}{T}\sum_{t=1}^{N} Y_{it},$$

$$X_{ki} = \frac{1}{T}\sum_{t=1}^{T} X_{kit} \qquad (k = 1, \ldots, K;\ i = 1, \ldots, N),$$

$$X_{i} = X_{1i}, \ldots, X_{Ki}). \tag{7.2}$$

The difference from the mean for any time period for any individual is:

$$Y_{it} - Y_{i}, \qquad X_{it} - X_{i} = [X_{1it} - X_{1i}, \ldots, X_{Kit} - X_{Ki}]. \tag{7.3}$$

For the error terms, the corresponding results are:

$$\alpha_i - \alpha_i = 0, \quad \eta_t - \eta, \quad \text{and} \quad e_{it} - e_{i},$$

where

$$\eta = \frac{1}{T}\sum_{t=1}^{T} \eta_t \text{ and } e_i = \frac{1}{T}\sum_{t=1}^{T} e_{it}.$$

Thus the individual effect α_i is eliminated by the regression

$$Y_{it} - Y_{i} = (X_{it} - X_{i})\,\beta + (\eta_t - \eta) + (e_{it} - e_{i}). \tag{7.4}$$

For the time effect η_t we can use time-period dummy variables by defining

$$\eta_t - \eta = d_t\gamma, \qquad (7.5)$$

where

$$d_t = (d_2, \ldots, d_T) \qquad d_j = 1, \text{ if } j = t \ (j = 2, \ldots, T), \\ = 0 \text{ otherwise,}$$

and γ contains the fixed-effect coefficients. This gives us:

$$Y_{it} - Y_i = (X_{it} - X_i)\,\beta + d\gamma + \xi_{it} \qquad (\xi_{it} = e_{it} - e_i), \qquad (7.6)$$

which, under the usual assumptions about X_{it} and e_{it} can be estimated by ordinary least squares to give the within-groups estimates β_w, γ_w.

We can also use the between-groups estimator, which is given by:

$$Y_i = X_i\,\beta + \alpha_i + e_i, \qquad (7.7)$$

where η is a constant and can be eliminated. This gives us the between-groups estimate β_B that will be biased if $E(\alpha_i \mid X_i) \neq 0$ as we expect. The generalized-least-square estimator of the random effect for α_i is then just a weighted average of the within-groups and between-groups estimators that can be determined from a regression of the form:

$$Y_{it} - (1 - \theta)\,Y_i = [X_{it} - (1 - \theta)\,X_i]\,\beta + \theta\,\alpha_i \\ + [d_t - (1 - \theta)]\,\gamma + [e_{it} - (1 - \theta)\,e_i], \qquad (7.8)$$

where $\theta = [\sigma_e^2/(\sigma_e^2 + T\sigma_a^2)]^{1/2}$ [Hausman and Taylor, 1981]. Unless $\theta = 0$, the random effects estimator β_R will be biased if $E(\alpha_i \mid X_{it}) \neq 0$ because of individual-specific omitted variables or worker heterogeneity. Only the within-groups estimator β_w will be unbiased and consistent in these circumstances.

Hausman and Taylor [1981] test for specification errors of the form $E(\alpha_i \mid X_{it}) \neq 0$ by comparing the within-groups estimator, which is unbiased and consistent, with the between-groups estimator, which is only unbiased and consistent under the null hypothesis that $E(\alpha_i \mid X_{it}) = 0$. This approach is useful when critical observed variables in the specified model are also individual-specific and do not vary over time. Estimation of the coefficients for these variables requires the random effects estimator if $E(\alpha_i \mid X_{it}) = 0$, or instrumental variables estimation if $E(\alpha_i \mid X_{it}) \neq 0$. This technique could be useful to evaluate cross-sectional estimates of the labour supply model using the Mincome data, but it is not necessary for reliable analysis of the experimental evidence.

Analysis of Experimental Panel Data

The within-groups estimator yields unbiased and consistent estimates provided that there is variation over time in the relevant variables. In Mincome and the other income maintenance experiments, this variation was introduced by assigning households to various income maintenance plans involving different guarantee levels and tax rates and adjusted for family size.[2] Hence, in principle, one can directly observe the labour supply response of household members to a change in the tax-transfer system (the within-groups estimate) as well as the labour supply responses across individuals facing different tax rates and income guarantee levels (the between-groups estimate).

The simplest method of estimating the effects of an income maintenance experiment on labour supply is to specify an analysis-of-variance (ANOVA) model, involving one or more dummy variables to distinguish the treatment group from the control group. Thus we could have:

$$Y_{it} - Y_i = T_i\,\beta_T + d\gamma + \xi_{it}, \qquad (7.9)$$

where

$$T_i = 1, \text{ if individual } i \text{ belongs to the experimental group;} \\ = 0, \text{ if individual } i \text{ belongs to the control group.}$$

Since the experimental group is assigned to plans with differing incentives and generosity, equation 7.9 can be expanded to allow for different effects by plan by specifying T_i to be a vector of qualitative variables representing differences between plans. Thus T_i could be a set of dummy variables identifying each plan as:

$$T_i = (P_{1i}, \ldots, P_{mi}), \qquad (7.9a)$$

where

$$P_{ji} = 1, \text{ if individual } i \text{ belongs to plan } j \ (j = 1, \ldots, m); \\ = 0 \text{ otherwise.}$$

The ANOVA model such as equation 7.9 has been a common starting point for statistical analysis of labour supply response for the income maintenance experiments in the United States. Most studies have simply set Y_i as pre-experimental, or baseline, labour supply, however, and Y_{it} as average labour supply during a portion of the experiment, typically the mid-year. (Hence time effects $d\gamma$ are dropped.) Studies of this type include Hall [1973] for the New Jersey experiment, Ashenfelter [1978] for the North Carolina rural experiment, and Robins and West [1978, 1980] for the Seattle-Denver experiment.

Alternatively, one could specify an analysis-of-covariance (ANCOVA) model that combines qualitative and quantitative variables to give a model of the form:

$$Y_{it} - Y_i = T_i \beta_T + (X_{it} - X_i)\, \beta_X + d\gamma + \xi_{it}. \qquad (7.10)$$

An analysis of the New Jersey experiment used a "spline series" that mixes qualitative and quantitative variables to describe the response surface as:

$$(T_i, X_{it} - X_i) = (S_{1i}, \ldots, S_{7i}), \qquad (7.10a)$$

where

S_{1i} = 1, if individual i belongs to the experimental group;
= 0 otherwise (control group);
$S_{2i} = S_{1i}(G_i - G_0)$, where G_i is the guarantee level assigned individual i and G_0 is the median experimental guarantee level;
$S_{3i} = S_{1i}\,(t_i - t_0)$, where t_i is the tax rate assigned individual i and t_0 is the median experimental tax rate;
$S_{4i} = \max(S_{2i}, 0)$;
$S_{5i} = \max(S_{3i}, 0)$;
$S_{6i} = S_{2i} \bullet S_{3i} - S_{4i} \bullet S_{5i}$; and
$S_{7i} = S_{4i} \bullet S_{5i}$.

Again comparison is made between the pre-experimental period and a portion of the experimental period, ignoring time effects $d\gamma$.

Other ANCOVA models might include a variety of control variables to capture differences in individual circumstances that affect labour supply response. Inclusion of these control variables is not necessary to obtain unbiased estimates of the experimental effects (e.g., β_T in equation 7.9) using an ordinary-least-square regression if the experimental assignment insures that T_i is uncorrelated with other determinants of labour supply response [Hausman and Wise, 1985]. Thus, in a random assignment of households to plans, variables such as household income would affect labour supply but would not affect the estimate of the experimental effect. If the assignment does not satisfy this condition, however, then control variables may be needed to obtain unbiased estimates.

As the experiments in the United States proceeded, three important issues emerged regarding non-random assignment and possible bias in the estimation of experimental effects: the Conlisk-Watts assignment model, the break-even status of families, and program participation and attrition. We consider these issues before turning to the empirical analysis of the labour supply response to the Mincome experiment.

The Conlisk-Watts Model and Bias

Mincome used the Conlisk-Watts assignment model [Conlisk and Watts, 1969] to increase the reliability of the experiment by favouring inexpensive observations over expensive ones (see Chapter 6). Keeley and Robins [1978] argue that the Conlisk-Watts assignment leads to bias in the estimation of the experimental effect on labour supply in equation 7.9 because labour supply is correlated with the assignment variables, especially normal family income. In particular, families supplying little labour prior to the experiment likely have low earnings and family income and are therefore less likely to be allocated to generous plans with high guarantee levels and/or low tax rates. Treatment T_i is not exogenous but is instead spuriously correlated with labour supply. Consequently, estimates of the experimental effect in equation 7.9 are biased.

Consider this argument for the simplest case of one experimental plan with a control group and two time periods, pre-experimental and experimental. The within-groups estimator of the experimental effect is derived from equation 7.9:

$$Y_{it} - Y_i = T_i \beta_T + d\gamma + \xi_{it} \qquad (t = 1, 2) \qquad (7.11)$$

where

d = 1 in period 2;
= 0 in period 1;
$\xi_{it} = e_{it} - e_i = e_{it} - (e_{i1} + e_{i2})/2$; and
$Y_i = (Y_{i2} + Y_{i1})/2$.

Keeley and Robins [1978] represent the non-random assignment by a model of the form:

$$T_i = \pi Y_{i1} + \tau_i, \quad Y_{i1} = \alpha_i + \eta_t + e_{i1}$$

$$= \pi\alpha_i + \pi\eta_\tau + \pi e_{i1} + \tau_i, \qquad (7.11a)$$

so that T_i is correlated with $\xi_{it} = e_{it} - (e_{i1} + e_{i2})/2$ in the equation above even if error τ_i is uncorrelated with ξ_{it}.

Keeley and Robins [1978] and Keeley [1981, pp. 124-25] argue that the only way to correct, at least partially, for non-random assignment is to include all assignment variables in the model in the form:

$$Y_{it} - Y_i = AT_i \beta_T + d\gamma + \xi_{it}, \qquad (7.12)$$

where

$AT_i = (A_1 T_i, \ldots, A_r T_i)$; and

$A_jT_i = T_i$, if the individual belongs to assignment category j ($j = 1, \ldots, r$);
$= 0$ otherwise.

This is equivalent to running a separate ANOVA model for each assignment category, imposing common time effects. Since payments under the experimental plans, and hence assignment, vary according to family size and composition as well as income prior to the experiment, there will be many assignment categories. This may seriously impair the ability of the model to generate statistically reliable estimates [Keeley, 1981, pp. 130-31].

Examination of equations 7.11 and 7.11a suggests that Keeley and Robins' argument ignores another source of bias when variables other than experimental treatment variables are included in the regression. Since labour supply prior to the experiment depends upon individual-specific factors (i.e., upon α_i), experimental assignment will also depend upon these factors. Thus α_i re-enters equation 7.11 through equation 7.11a. Since α_i is unobserved it may bias estimates of the effects of other included variables on labour supply response in an undetermined manner. Hence control for non-random assignment, as in equation 7.12, takes on added importance.

The Break-Even Status

The assignment model determines a guarantee level G and tax rate t for each family depending upon the plan to which the family is assigned and, in the case of the guarantee, family size. Once the assignment is made, however, families may decide not to participate in the experiment. Non-participation is a prominent feature of all the income maintenance experiments and deserves attention as a possible source of bias in estimating experimental effects.

One source of non-participation is the break-even status, which refers to family pre-experimental income y relative to the break-even level of income $B = G/t$ for the plan to which they are assigned. If $y \leq B$, a family is at or below the break-even point and will face a different tax-transfer system during the experiment with respect to its pre-experimental labour supply decisions. This is the classic case for marginal analysis of labour supply decisions, comparing experimental to pre-experimental labour supply as the family's budget constraint is shifted. If $y > B$, however, a family is above the break-even level and, at its prevailing (pre-experimental) labour supply, will not be affected by the experimental treatment. Labour supply decisions at the margin will not change (or, at least, changes will not be experimentally induced) as in the classic case. We would therefore expect labour supply response to differ depending upon the break-even status ($y \leq B$ or $y > B$).

By reducing labour supply such that income falls below the break-even level of the assigned plan, a family can receive payment and, in some cases, be better off than remaining outside the plan. Ashenfelter [1980] presents a simple model for a family with one worker to illustrate this point. Let $E(w_a, v)$ be the minimum unearned income required by a worker with after-tax wage rate w_a to reach utility level v. If t is the tax rate under the experimental plan, then participation requires that the family be better off under the plan or that the guarantee compensate for the effects of the tax:

$$E[(1-t)\,w, v] - E[w, v] < G. \qquad (7.13)$$

He approximates the left-hand side of equation 7.13 by a second-order Taylor expansion to obtain the condition for participation to be:

$$twh - \frac{1}{2}\, s(tw)^2 < G$$

or

$$\frac{G}{t} - wh > -\frac{1}{2}\, stw^2, \qquad (7.14)$$

where h is pre-experimental hours worked and $s = \partial h/\partial w = \partial^2 E/\partial w^2$ is the compensated wage effect. A family below the break-even income level always participates if $s > 0$ as expected, since

$$B - y = \frac{G}{t} - wh > -\frac{1}{2}\, stw^2,$$

whereas a family above the break-even level may participate if $B - y < 0$ but the condition in equation 7.14 is satisfied. For a family above the break-even level, the participation condition can be rewritten as:

$$\frac{1}{2}\, stw^2 - (y - B) > 0, \qquad (7.15)$$

indicating that families near the break-even point are more likely to participate since $y - B$ is small.

This analysis suggests two simple measures of the break-even status used in the analysis of participation in the Seattle-Denver experiment [Keeley et al., 1978]:

FABOVE = 1, if $y - B > 0$ (above the break-even level);
= 0, if $y - B \leq 0$ (at or below the break-even level); and

$$EARNABV = y - B, \text{ if } y - B > 0 \text{ (earnings above the break-even level)};$$
$$= 0, \text{ if } y - B < 0. \qquad (7.16)$$

Keeley [1981, p. 131] argues, however, that the issue of differential response by break-even status is complicated by the non-random assignment. The assignment depends upon normal pre-experimental income and family size, which both affect the break-even status as well as labour supply. Hence the break-even status should be interacted with the experimental treatment variable(s) in the same manner as other assignment categories, based on income and family size, in equation 7.12.[5]

Ashenfelter [1980, 1983] provides a clear and concise statistical model of the participation decision and its relation to labour supply, ignoring non-random assignment. Equation 7.14 implies that the income level below which families participate is:

$$wh^* = \frac{G}{t} + \frac{1}{2}\, stw^2 = B + \frac{1}{2}\,(s\frac{w}{h})\,twh \qquad (7.17)$$

or

$$wh^* = \frac{B}{1 - \frac{1}{2}\,te}, \qquad (7.18)$$

where $e = s\ w/h$ is the compensated wage elasticity with respect to labour supply. If income or earnings is lognormally distributed with mean μ and variance σ^2, then the probability of participation is:

$$\text{Prob}\,(\ln\,(wh^*) - \ln wh > 0)$$
$$= \text{Prob}\left(\frac{\ln B + \frac{1}{2}\,te - \mu}{\sigma} > 0\right)$$
$$= N\left(\frac{\ln B + \frac{1}{2}\,te - \mu}{\sigma}\right),$$

where $N(\bullet)$ is the normal distribution function. This is a simple probit model that may be written as:

$$PTC = 1, \text{if } \frac{\ln B + \frac{1}{2}\,te - \mu}{\sigma} > 0,$$
$$= 0, \text{if } \frac{\ln B + \frac{1}{2}\,te - \mu}{\sigma} \leq 0. \qquad (7.19)$$

If there is no labour supply response among experimental families, then $e = 0$ and participation is determined solely by the break-even status, B. The tax term $1/2\ te$ represents the distinct behavioural component of participation arising from labour supply response.

Ashenfelter allows experimental mean income μ to vary according to pre-experimental earnings and normal income cell because families in the experiments were screened on the basis of these factors. For control families, this means that the logarithm of earnings during the experiment depends on these same factors. Thus he fits three equations to data from the Seattle-Denver Income Maintenance Experiment for double-headed families: a probit model explaining participation for experimental families; an ordinary-least-square model explaining the logarithm of earnings for control families; and a combined model for experimental and control families. Only pre-experimental earnings and, in the case of experimental families, the break-even level are significant. The coefficient of the tax term gives an estimate of the compensated wage elasticity of 0.13 in the combined sample and 0.23 in the experimental probit model. This is consistent with labour supply results reported in Chapters 4 and 5, but the result is not statistically significant.

An unpublished study of participation in the Mincome experiment by Sabourin [1985] corroborates Ashenfelter's results. For experimental families only the break-even level of income affects participation and the elasticity of labour supply is not significantly different from zero. Both studies indicate that participation is primarily determined by eligibility (break-even status) rather than choice (labour supply response) because labour supply response is small. This does not necessarily imply that the break-even status does not affect labour supply response in Mincome, however, and we account for differences in behaviour by break-even status in subsequent empirical analysis.

Attrition

Another potentially important source of non-participation in the income maintenance experiment, or any panel data set, is attrition. Many families, both in the experimental and control groups, left the experiment before it was completed. These decisions, like those related to the break-even status, may not be random since families have different financial incentives to remain in the experiment, depending upon their income and the generosity of the plan. In particular, the payment to a family in the Mincome group based on its pre-experimental labour supply would be:

$$PAY = t\,(B - y) = G - ty, \text{ if } B - y > 0,$$
$$= 0, \text{ if } B - y \leq 0, \qquad (7.20)$$

which varies directly with the break-even status, $B - y$, and the tax rate, t.

Robins and West [1978] provide the only assessment to date of the effect of attrition on labour supply response. They find that participation increases as the financial incentives provided by the experiment improve. Correcting for attrition as selection bias in a manner similar to Heckman [1979] results in larger substitution effects and smaller income effects among participants. Their results imply that labour supply response is affected by attrition from the panel data for the Seattle-Denver Income Maintenance Experiment. It is not clear how the experimental effect will change, however, since labour supply response will be smaller for those whose income is increased by the experiment (the income effect) but larger for those whose after-tax wages decline (the substitution effect).

Empirical Results for Mincome

In this section we provide some basic statistical results for labour supply response in the Mincome data for intact families (those who had no change in marital status during the experiment) from Winnipeg. We begin with some basic descriptive statistics before turning to the analysis of panel data and the special problems of assignment and participation in the experimental data.

Basic Descriptive Results for the Mincome Sample

The Mincome sample of intact families from Winnipeg consists of 1,187 families, of which 650 (or 55 per cent) are double-headed, 364 (or 31 per cent) are female single-headed, and 173 (or 15 per cent) are male single-headed. Of these 1,187 families, 612 (or 52 per cent) were assigned to the control group. The remaining 575 families were assigned to one of eight plans as shown in Table 7-1. The plans vary according to guarantee level, adjusted according to family size, and tax rate to produce considerable variation in the financial incentive to work.

Variations in labour supply are represented by hours worked on an annual basis in Table 7-2 for three groups – all males, females in double-headed families (or wives), and females in single-headed families (or single female heads). Married and single females are distinguished, while married and single males are combined, to reflect the main categories studied in the labour supply literature reviewed in Chapters 2 and 3. The evidence in Table 7-2 is difficult to interpret because there are several different plans, which should have different effects on labour supply, and because there is very likely substantial random variation in the data. The data show declining hours worked from the first (or pre-experimental) year to the other (experimental) years for most plans but also for the control group. It is by no means easy to see whether the pattern in hours worked between the first year and the remaining years for the experimental group differs from that of the control group.

Basic Analysis of the Mincome Panel Data with ANOVA and ANCOVA Models

We can summarize the evidence in Table 7-2 in various ways using the simple ANOVA and ANCOVA models for

Table 7-1

Number of Intact Families from Winnipeg Assigned to the Mincome Plans, 1974-77

	Double-headed families	Single-headed families		All families	
		Men	Women	Men	Women
Plans[1]					
1) $G = \$3{,}800$; $t = 0.35$	29	21	19	50	48
2) $G = \$4{,}800$; $t = 0.35$	39	8	25	47	64
3) $G = \$3{,}800$; $t = 0.50$	41	17	43	58	84
4) $G = \$4{,}800$; $t = 0.50$	59	6	15	65	74
5) $G = \$5{,}800$; $t = 0.50$	35	9	21	44	56
6) $G = \$3{,}800$; $t = 0.75$	40	20	29	60	69
7) $G = \$4{,}800$; $t = 0.75$	24	7	15	31	39
8) $G = \$5{,}800$; $t = 0.75$	35	3	15	38	50
Total experimental group	302	91	182	393	484
Control group	348	82	182	430	530
Experimental and control groups	650	173	364	823	1,014

[1] Actual plan guarantees are adjusted according to family size.

Table 7-2

Number of Hours Worked on an Annual Basis, by Plan and Year[1] of the Mincome Experiment, 1974-77

	Year 1	Year 2	Year 3	Year 4
Men:				
Plans				
1) $G = \$3{,}800; t = 0.35$	1,653.2	1,663.1	1,547.7	1,664.4
2) $G = \$4{,}800; t = 0.35$	1,627.3	1,437.5	1,412.4	1,557.8
3) $G = \$3{,}800; t = 0.50$	1,507.5	1,191.0	1,213.6	1,114.8
4) $G = \$4{,}800; t = 0.50$	1,888.5	1,681.9	1,577.0	1,489.4
5) $G = \$5{,}800; t = 0.50$	1,900.6	1,581.2	1,341.3	1,521.7
6) $G = \$3{,}800; t = 0.75$	1,499.2	1,409.9	1,506.1	1,532.3
7) $G = \$4{,}800; t = 0.75$	1,798.5	1,670.1	1,605.2	1,600.9
8) $G = \$5{,}800; t = 0.75$	1,771.4	1,658.8	1,513.9	1,570.5
Control group	1,720.5	1,679.3	1,567.9	1,582.0
Experimental and control groups	1,711.0	1,580.5	1,704.0	1,472.0
Women in double-headed families (wives):				
Plans				
1) $G = \$3{,}800; t = 0.35$	511.6	545.4	354.1	351.0
2) $G = \$4{,}800; t = 0.35$	502.3	559.4	438.0	332.8
3) $G = \$3{,}800; t = 0.50$	443.2	530.5	385.5	277.2
4) $G = \$4{,}800; t = 0.50$	415.2	529.7	416.5	529.8
5) $G = \$5{,}800; t = 0.50$	523.2	478.0	444.1	351.0
6) $G = \$3{,}800; t = 0.75$	794.8	442.9	454.5	370.8
7) $G = \$4{,}800; t = 0.75$	526.0	486.3	635.3	469.6
8) $G = \$5{,}800; t = 0.75$	526.0	593.1	460.0	703.3
Control group	561.0	625.7	587.0	682.6
Experimental and control groups	542.9	559.1	489.8	516.2
Female single heads:				
Plans				
1) $G = \$3{,}800; t = 0.35$	1,012.1	1,058.7	876.4	969.5
2) $G = \$4{,}800; t = 0.35$	1,445.8	1,247.3	1,202.3	1,150.7
3) $G = \$3{,}800; t = 0.50$	1,492.2	1,285.0	1,017.8	1,006.9
4) $G = \$4{,}800; t = 0.50$	1,323.4	1,342.7	696.3	770.4
5) $G = \$5{,}800; t = 0.50$	1,532.5	1,033.9	1,241.9	1,167.3
6) $G = \$3{,}800; t = 0.75$	1,058.0	1,103.0	952.6	1,055.9
7) $G = \$4{,}800; t = 0.75$	1,510.7	1,522.1	1,340.6	1,571.0
8) $G = \$5{,}800; t = 0.75$	1,353.3	1,279.1	1,169.7	1,136.8
Control group	1,088.3	982.4	873.0	926.5
Experimental and control groups	1,221.9	1,153.8	1,002.4	1,027.8

[1] Year 1 consists of the first, or baseline or pre-experimental, survey in 1974. Year 2 consists of the second to fourth surveys generally conducted in 1975. Year 3 consists of the fifth through eighth surveys generally conducted in 1976. Year 4 consists of the ninth to eleventh surveys generally conducted in 1977. Since interview dates vary from respondent to respondent, all data is prorated to a 365-day period to reflect annual hours worked.

panel data described by equations 7.9, 7.9a, and 7.10a. The results of these regressions are presented in Table 7-3 for men, married women, and single female heads. The first regression (column 1) for each group simply compares the mean difference in annual hours worked between the experimental and control groups.[4] For males, the difference is negative, as expected, and statistically significant at the 5-per-cent level; those in the experimental group work 92 hours less per year on average. For wives, the experimental effect is also negative, indicating a reduction of

25 hours per year or 4.6 per cent, but statistically insignificant. For single female heads, the effect is negative and significant, indicating that those in the experimental group work 100 hours less per year, or 8.2 per cent, on average.

The experimental effect remains negative but becomes insignificant when fixed-time effects are included in equation 7.9 (column 2). Inclusion of the different experimental effects through dummy variables for each plan (equation 7.9a) or a spline series (equation 7.10a) yields generally insignificant experimental effects provided that the time effects are included. Hence the simple evidence from the ANOVA and ANCOVA models in Table 7-3 suggests that the Mincome experiment reduced annual hours worked for men, single female heads, and married women, but that the effects were small and not statistically significant.

The regressions in Table 7-3 have very little explanatory power, explaining less than 5 per cent of the total variation in annual hours worked. Provided that the experimental effect is uncorrelated with omitted explanatory variables, this poses no problem in estimating the experimental effect. We have noted, however, that the experimental effect was not randomly assigned and that participation in the experiment is related to labour supply behaviour. Hence we should examine the effect of assignment and participation on the experimental effect of Mincome.

Assignment and Participation Effects in Mincome

To examine the effect of non-random assignment on hours worked we follow Keeley and Robins [1978] and include assignment variables in the ANOVA and ANCOVA regressions presented in the previous section. The assignment variables used are the five normal income cells[5] and the family size index to represent family size and composition. The assignment variables are allowed to interact with the treatment variable, as in equation 7.12, and on their own.

To examine the effect of participation on hours worked, we adopt two procedures. First, we construct simple variables to represent the break-even status and attrition from the experiment. The variables representing the break-even status are *FABOVE* and *EARNABV* from equation 7.16. To examine the effect of attrition we include those households who left the experiment, using a dummy variable (*ATT3* in year 3 and *ATT4* in year 4) to denote when they left. Second, we use a simple participation model based on the financial incentives created by the experiment to correct for selection bias arising from attrition.

Table 7-4 presents the first set of results. Columns 1 to 3 for each group (men, married women, and single female heads) include assignment variables and variables to capture the break-even status, but they exclude the participants who quit the experiment. Column 1 follows equation 7.12 faithfully, interacting the assignment and break-even status variables with the treatment variable. The assignment and break-even status variables are insignificant individually and as a group. The experimental effect remains insignificant as well. Note that the coefficient on the treatment variable, *S*1, cannot be interpreted as the experimental effect because of the presence of the interaction terms.

Columns 2 and 3 of each group introduce the assignment and break-even status variables independently. The absence of interaction terms facilitates comparison with Table 7-3. The results are very similar. Hours worked for males are estimated to decline by 15 hours, or 0.9 per cent, as a result of the experiment in both Tables 7-3 and 7-4 when the time effects are considered. Hours worked for wives are estimated to decline by 15 hours, or 2.9 per cent, in Table 7-4 as opposed to 13 hours in Table 7-3. Hours worked for single female heads are estimated to decline by 49 hours, or 4.1 per cent, in Table 7-4 as opposed to 47 hours in Table 7-3. Each of these experimental effects remains statistically insignificant. The assignment and break-even status variables are also statistically insignificant, individually and as a group, in each case.

Columns 4 and 5 for each group in Table 7-4 expand the data set to include participants who quit during the experiment.[6] Although the experimental effect is somewhat larger for males and single females in this expanded sample, it remains statistically insignificant. The dummy variable *ATT3* indicating attrition after the second survey year, or first experimental year, is significant and negative for males in both columns and the two attrition variables, *ATT3* and *ATT4*, are jointly significant ($F = 3.03$ and 3.01). Thus males who quit the experiment have significantly fewer hours worked than males who remain throughout the experiment. Attrition effects are statistically insignificant for both married and single females.

Conclusion

The Mincome experiment provides unique Canadian panel data on labour supply behaviour in which households have been assigned a variety of tax rates and income guarantees. This chapter serves as an introduction to the data and to some basic methods of evaluating the labour supply effects from the experiment. It also introduces some unique problems associated with the evaluation of experimental panel data, particularly assignment and participation.

The results from this chapter are summarized in Table 7-5 and compared to results from the four U.S. experiments in

Table 7-3

Estimates of Annual Hours Worked Using ANOVA and ANCOVA Models, Based on Mincome Data (Equations 7.9, 7.9a, and 7.10a)[1]

	Men				Married women				Single female heads			
	Equation 7.9		Equation 7.9a	Equation 7.10a	Equation 7.9		Equation 7.9a	Equation 7.10a	Equation 7.9		Equation 7.9a	Equation 7.10a
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Variables:												
Intercept	46.12*	123.74*	123.74*	123.74*	12.18	25.10	25.10	25.10	53.62*	99.41*	99.41*	99.41*
S1	−92.23*	−14.61	. . .	−12.67	−25.32	−13.31	. . .	−0.53	−100.36*	−47.29	. . .	−95.19
S2	. . .	. . .	. . .	0.01	. . .	. . .	. . .	0.005	. . .	. . .	. . .	0.00
S3	. . .	. . .	. . .	−148.69	. . .	. . .	. . .	51.68	. . .	. . .	. . .	−311.19
S4	. . .	. . .	. . .	−0.10	. . .	. . .	. . .	−0.03	. . .	. . .	. . .	0.12
S5	. . .	. . .	. . .	212.95	. . .	. . .	. . .	−182.48	. . .	. . .	. . .	933.79
S6	. . .	. . .	. . .	−0.14	. . .	. . .	. . .	−0.02	. . .	. . .	. . .	0.41
S7	. . .	. . .	. . .	0.41	. . .	. . .	. . .	0.27	. . .	. . .	. . .	−1.15
T2	. . .	−108.24*	−108.24*	−108.24*	. . .	−3.34	−3.34	−3.34	. . .	−64.19	−64.19	−64.19
T3	. . .	−170.41*	−170.41*	−170.41*	. . .	−45.62	−45.62	−45.62	. . .	−121.68*	−121.68*	−121.68*
T4	. . .	−187.07*	−187.07*	−187.07*	. . .	−25.82	−25.82	−25.82	. . .	−110.73	−110.73	−110.73
P1	. . .	. . .	−16.37	. . .	. . .	. . .	−27.95	. . .	. . .	. . .	6.76	. . .
P2	. . .	. . .	13.34	. . .	. . .	. . .	1.62	. . .	. . .	. . .	−54.91	. . .
P3	. . .	. . .	0.04	. . .	. . .	. . .	9.37	. . .	. . .	. . .	−88.24	. . .
P4	. . .	. . .	−27.02	. . .	. . .	. . .	−0.15	. . .	. . .	. . .	−71.75	. . .
P5	. . .	. . .	−100.65*	. . .	. . .	. . .	−43.45	. . .	. . .	. . .	−58.30	. . .
P6	. . .	. . .	29.94	. . .	. . .	. . .	−56.95	. . .	. . .	. . .	−26.23	. . .
P7	. . .	. . .	16.89	. . .	. . .	. . .	15.18	. . .	. . .	. . .	60.67	. . .
P8	. . .	. . .	0.19	. . .	. . .	. . .	5.07	. . .	. . .	. . .	−47.72	. . .
R^2	0.01	0.03	0.04	0.04	0.001	0.003	0.005	0.004	0.01	0.02	0.03	0.03
F	14.95*	10.75*	4.38*	4.38*	1.35	0.98	0.57	0.48	7.73*	3.18	1.36	1.60
F^1	14.95*	0.25	0.69	0.87	1.35	0.25	0.32	0.16	7.33*	1.06	0.42	0.62
n	1,284	1,284	1,284	1,284	1,172	1,172	1,172	1,172	584	584	584	584

*Denotes statistical significance at the 5-per-cent level.

F^1 refers to the test for the joint significance of all terms representing an experimental effect, for example, all S_i or all P_i.

[1] Regression method is OLS.

Table 7-4

Estimates of Annual Hours Worked Using ANCOVA Models, Based on Mincome Data (Equation 7.12)[1]

	Men					Married women					Single female heads				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Variables:															
Intercept	120.70*	120.71*	112.97	136.50*	128.51*	25.10	20.69	19.46	24.02	23.70	100.10*	99.16	105.94	70.59	79.42
S1	2.43	−15.13	−15.79	−27.96	−31.63	−7.94	−15.67	−2.97	−14.28	28.26	20.01	−49.88	−105.09	−64.82	−119.36*
S2	...	...	0.004	...	0.03	...	...	0.00	...	0.02	...	...	−0.01	...	−0.04*
S3	...	...	−154.84	...	−418.66	...	...	51.68	...	271.85	...	...	−346.19	...	−256.42
S4	...	...	−0.09	...	−0.09	...	...	−0.03	...	−0.08	...	...	0.14	...	0.10
S5	...	...	220.65	...	472.97	...	...	−187.40	...	−527.17	...	...	986.91	...	821.92
S6	...	...	−0.15	...	−0.15	...	...	−0.02	...	0.15	...	...	0.42	...	0.42
S7	...	...	0.39	...	0.19	...	...	0.27	...	0.39	...	...	−1.18	...	−0.76
T2	−105.47*	−104.12*	−103.86*	−96.46*	−96.00*	−3.34	−1.82	−1.80	17.28	17.19	−64.64	−63.12	−63.60	−16.73	−16.44
T3	−166.80*	−165.46*	−165.20*	−188.02*	−187.92*	−45.62	−44.10	−44.09	−33.93	−33.96	−122.52*	−121.00*	−121.47*	−85.10	−84.66
T4	184.34*	−183.00*	−182.74*	−181.56*	−181.52*	−25.82	−24.30	−24.28	−21.40	−28.10	−111.50	−109.98	−110.46	−52.95	−51.09
NIC2	...	1.59	3.18	8.00	7.57	...	2.56	3.31	−10.75	−5.91	...	6.36	1.45	6.46	5.62
NIC3	...	−0.86	1.07	−0.37	−1.45	...	0.47	3.53	−10.27	−5.25	...	6.39	6.87	15.10	16.33
NIC4	...	−0.11	6.30	1.33	−0.54	...	1.04	2.67	−5.86	−6.78	...	5.25	2.08	12.48	14.19
NIC5	...	−1.46	−2.34	−6.95	−12.77	...	−0.17	2.39	−11.66	−6.72	...	17.42	−30.00	26.87	9.05
FABOVE	...	3.82	−10.95	2.63	−4.10	...	5.54	2.66	−12.59	−14.66	...	24.22	1.95	42.20	18.26
EARNABV	...	0.00	0.00	0.00	0.00	...	0.00	0.00	0.00	0.00	...	0.00	0.00	0.00	0.00
FSI	...	−0.01	0.04	−0.08	0.02	...	0.02	0.01	0.03	0.00	...	−0.06	−0.13	−0.10	−0.22
S1*FSI*NIC2	−0.13	...	...	...	...	0.20	...	...	...	...	−1.15	...	...	...	...
S1*FSI*NIC3	−0.35	...	...	...	...	0.32	...	...	...	...	−1.44	...	...	...	...
S1*FSI*NIC4	−0.37	...	...	...	...	0.35	...	...	...	...	−1.07	...	...	...	...
S1*FSI*NIC5	−0.09	...	...	...	...	0.05	...	...	...	...	0.23	...	...	...	...
S1*FABOVE	150.82	...	...	...	...	−88.99	...	...	...	...	83.57	...	...	...	...
ATT3	...	...	...	−71.21*	−71.09*	...	...	...	−35.28	−33.85	...	...	...	−20.58	−23.71
ATT4	...	...	...	−24.89	−25.47	...	...	...	−7.38	−6.02	...	...	...	−6.78	−11.13
R^2	0.035	0.031	0.035	0.034	0.037	0.01	0.003	0.004	0.004	0.008	0.030	0.022	0.028	0.015	0.020
F	4.59*	3.72*	2.70*	4.81*	3.60*	1.24	0.36	0.28	0.42	0.65	1.73	1.15	0.94	0.97	0.86
F^a	0.72	0.24	0.76	1.04	1.00	1.25	0.30	0.17	0.31	1.02	0.81	1.11	0.64	2.58	0.92
n	1,280	1,280	1,280	1,806	1,306	1,172	1,172	1,172	1,508	1,508	580	580	580	815	815

*Denotes statistical significance at the 5-per-cent level.

F^a refers to the test for the joint significance of all terms representing an experimental effect, for example, all S_i or all P_y.

[1] Regression method is OLS.

Robins [1985]. The labour supply response from Mincome is lower than any of the results from the United States for males and wives, although similar to the Gary results for single female heads. The results from Mincome imply a labour supply response of 0.8 to 1.6 per cent for males compared to a combined response of 5 per cent from the U.S. experiments; a response of 2.4 to 3 per cent for wives compared to 21.1 per cent from the United States; and a response of 3.8 to 5.3 per cent for single female heads compared to 13.2 per cent from the United States.

This comparison can be misleading, however. The combined U.S. results are dominated by the Seattle-Denver experiment which had the largest sample, the most complex design, and the largest reported experimental effects. The other U.S. experiments found smaller and statistically insignificant effects as we have for Mincome. Indeed, our results are quite similar to the findings of the first two urban experiments, New Jersey and Gary, and differ markedly only from the Seattle-Denver results. Our results thereby agree with the assessment of the U.S. experiments in Basilevsky and Hum [1984].

Comparison is also complicated by the different combinations of guarantee levels and tax rates offered to participants in each experiment. Reported experimental effects aggregate the effects from a series of plans that differ in each experiment. In this sense every experiment is unique and simple comparisons of the experimental effects, or the combined effects as in Robins [1985], must be used cautiously. In particular, it would be unwise to use the experimental effects summarized in Table 7-5 in the analysis of current social policy questions, since the policy options under consideration are unlikely to conform to the particular mixture of plans from one, or a combination, of the experiments.

Table 7-5

Summary of the Experimental Effects, Based on Mincome and Four U.S. Experiments

	Average labour supply response based on annual hours worked		
	Men	Married women	Single female heads
Experiment:			
Mincome	−13 to −28 (0.8 to 1.6)[1]	−13 to −16 (2.4 to 3.0)	−47 to −63 (3.8 to 5.3)
New Jersey	−34.2	−55.7	. . .
Rural communities	−56	−178	. . .
Gary	−35.4	−57.6	−37.1
Seattle-Denver[2]	−112.8	−141.2	−163.3
All U.S. programs	−89 (5.0)	−117 (21.1)	−121 (13.2)

1 Percentage change in parentheses.
2 Three-year experiment only.
Source Regressions including time effects in Tables 7-3 and 7-4 for Mincome; Robins [1985] for the U.S. experiments.

Comparison of results and policy application is possible, however, when the experimental panel data are used to estimate structural labour supply models. Wage and income elasticities provide a common basis for the evaluation of experimental results that can also form the basis for the evaluation of current social policy alternatives. Thus we now turn to the estimation of wage and income elasticities from structural labour supply models and to further evaluation of the empirical results reviewed in Chapters 2 to 4.

8 Structural Labour Supply Estimates from the Mincome Experiment

The estimates of the labour supply response based on the Mincome data provided in the previous chapter cannot be easily used for general social policy analysis. They cannot even be easily used for the assessment of a current guaranteed annual income program such as the Universal Income Security Program proposed by the Macdonald Commission because the estimates refer to the specific effect of experimental programs whose features are unlikely to correspond to such proposals. The best way to provide results for general social policy analysis, from an economic standpoint, is to use the Mincome data to estimate a specific structural model of labour supply such as the conventional static labour supply model of Chapter 2.

In this chapter, we provide estimates of the structural labour supply from the Mincome data. We first estimate the labour supply model from the Mincome pre-experimental baseline data, followed by estimates from the experimental panel data including the baseline data. The analysis is organized to permit us to assess the empirical literature reviewed in Chapters 2 to 4 at least insofar as it applies to the low-income population. The baseline data provide a non-experimental, cross-sectional data set of the sort used in most of the empirical work on labour supply. A variety of estimation techniques, representative of the approaches taken in the literature, are used to estimate the baseline results and assess the robustness of the estimates. We then turn to the experimental panel data to provide a further comparison and what we feel are superior estimates. In conclusion, we summarize our findings and evaluate the empirical literature on labour supply once more.

Structural Estimates from the Mincome Baseline Data

A natural place to begin investigation of labour supply in the Mincome data is the pre-experimental baseline data. They provide a rich source of cross-sectional data that have already been used for analysis of the labour supply behaviour of males [Ehnes and Simpson, 1983], female household heads [Prescott et al., 1986], and double-headed households [Simpson et al, 1987]. The results from the baseline data provide a convenient basis for comparison of results from cross-sectional data, so commonly used in the literature to date, and experimental panel data.

Previous studies of the Mincome baseline data provide some initial estimates with fairly sophisticated treatment of such important econometric issues as truncation. These studies are limited, however, to a single specification and a single estimation technique. Our approach, in the tradition of DaVanzo et al. [1976] and Mroz [1987], assesses the sensitivity of the results under a variety of specifications and estimation techniques. It goes beyond previous work in two respects. First, we consider all groups – men and women in single- and double-headed households – in a fully specified family labour supply model. Both DaVanzo et al. and Mroz ignored cross-wage effects, which may be an important consideration for married women in particular. Second, we extend the sensitivity analysis in the next section to consider alternative estimates from experimental panel data that include the baseline data.

We begin with the conventional static model of family labour supply specified in Chapter 2 (equations 2.1 to 2.9). The model is then estimated by a variety of methods, representing increasingly sophisticated treatment of the most prominent econometric problems discussed in Chapters 2 and 3:

- Ordinary-least-square (OLS) estimation of hours worked using observed wage rates, thus restricting the sample to those with positive hours worked;

- OLS estimation of hours worked, including in the sample those with zero hours, using an imported wage rate estimated by OLS from the sample with observed wages.

- Tobit estimation of hours worked, including in the sample those with zero hours, using an imputed wage rate estimated by OLS.

- Estimation of hours worked for those with positive hours, correcting for sample selection bias and using an imputed wage rate estimated by OLS.

- Tobit information of hours worked, using an imputed wage rate estimated by Heckman's [1979] procedure from the sample with observed wages correcting for sample selection bias.

- Estimation of hours worked for those with positive hours, correcting for sample selection bias, and using an imputed wage rate corrected for sample selection bias.

The first two methods represent first-generation approaches to the problem reviewed in Chapter 2. The last four methods represent second-generation methods reviewed in Chapter 3. The final two approaches correspond to the solutions in equations 3.9 and either 3.10 (for the Tobit model) or 3.11 (for the selectivity model or the "generalized Tobit" model [Mroz, 1987]) for the problem specified in equations 3.4 to 3.8.

In addition, we repeat these estimates accounting for individual tax rates using the linearized-budget-constraint approach of Hall [1973]. That is, we determine the appropriate marginal tax rate and virtual income for each individual under the 1974 Manitoba tax regime and adjust wage rates and unearned income to reflect the position of the individual after taxes under the assumption that the prevailing marginal tax rate applies to all earnings. Unlike Hall, we treat the after-tax wage rate as endogenous and use an instrumental variable to avoid bias arising from the positive correlation between hours worked and the marginal tax rate in the personal tax system.

Our approach covers much of the same ground as Mroz's [1987] very useful sensitivity analysis for married women. Like Mroz we ignore Hausman's [1981] non-linear-budget-constraint approach to the treatment of taxes because simultaneous treatment of truncation problems and non-linear budgets is simply too complex. Like Mroz, we treat after-tax wages as endogenous rather than exogenous. Because our sample is restricted to the Manitoba tax system, we are able to specify the tax regime more thoroughly to include provincial as well as federal tax credits and liabilities.

We begin with estimates of wage equations for males and females, using OLS with and without correction for sample selection for four different specifications. Following Mroz, we include direct measures of labour force experience, reported years of working experience since leaving school, in one set of wage equations. We include only an indirect measure of experience, age minus years of school minus five, in the other wage equations. Half of the equations use very basic specifications – education, experience, and experience squared (and age where direct experience is used) – while the remaining equations add second-order terms in education and age. The equations are typical of the specifications used in the labour supply literature to impute wage rates and correspond as closely as possible to Mroz's specifications up to second-order terms.

Our dependent variable is the wage rate for hourly rated jobs. This avoids the problem of division bias that arises when hours worked are used to determine the wage rate [Borjas, 1980].[1] In our experience, it also avoids the problem of editing errors and inconsistent treatment of wage rate calculations. Most studies have been unable to use only hourly rated wages because of data limitations. In these studies, the problem of division bias may be circumvented by using an imputed wage, as is common in the second generation of studies of labour supply in Chapter 3, provided that the characteristics used in the imputed wage equation are uncorrelated with errors in hours worked. In practice this provision is difficult to ensure so that division bias may be present in the studies that do not use hourly rated wages.

The results, provided in supplementary Table 1,[2] generally conform to expectations. Education increases wages for both males and females. The rate of return to education after tax is only about 3 per cent for males and 5 per cent for females. These low rates of return relative to the entire population may simply reflect income truncation bias in the Mincome sample [Hausman and Wise, 1977]. Experience increases earnings with the usual declining effect with increasing experience (i.e., the experience squared term has a negative coefficient) regardless of the experience measure used. Sample selection bias is significant in general, and all equations have significant explanatory power.

These wage equations permit a variety of imputed wages to be calculated and used to estimate hours worked for the full sample, including those with zero hours, using OLS and Tobit regressions. The imputed wages are also used to estimate hours worked for the sample with positive hours, correcting for sample selection bias. The full results, with and without allowance for taxes, are provided in supplementary Table 2. Only the more salient results of that table are presented and discussed below.

The labour supply results in supplementary Table 2 permit us to calculate labour supply elasticities and present a compact summary of the salient results in Table 8-1. The latter concentrates on the estimates of the uncompensated wage elasticity implied by supplementary Table 2 because they vary to a much greater extent than other crucial variables. Table 8-1 also summarizes the other crucial variables, the income and cross-wage effects, by providing the range of estimates for the income and cross-wage elasticities implied by the results in supplementary Table 2. The income elasticity is uniformly small, often insignificant, and occasionally positive contrary to expectations. Thus a large (small) uncompensated wage elasticity in Table 8-1 implies a large (small) compensated wage elasticity.

There are many large wage elasticities in Table 8-1. Indeed, the range of results suggests that very little consensus

Table 8-1

Uncompensated Wage Elasticity Estimates and Range of Income Elasticity and Cross-Wage Elasticity Estimates for Various Labour Supply Models from the Mincome Baseline,[1] 1974

	Estimates of annual hours worked, without adjustment for taxes			Estimates of annual hours worked, with adjustment for taxes		
	OLS	Tobit model	Generalized Tobit model[2]	OLS	Tobit model	Generalized Tobit model[2]
Men in double-headed families:						
Wage measure						
Observed wage	–0.25	...	...	–0.18	...	...
W(1) – indirect experience, OLS	2.48*	–0.10	1.94*	4.73*	–0.04	4.16
W(2) – indirect experience, second-order terms, OLS	3.05*	–0.17	2.51*	–6.30*	–1.79	4.44*
W(3) – direct experience, OLS	0.85*	1.22*	0.79*	0.89*	1.38*	1.11*
W(4) – direct experience, second-order terms, OLS	1.03*	0.75*	1.04*	1.18*	1.43*	1.46*
W(5) – indirect experience, selectivity adjusted	6.73*	–0.94	5.27*	4.11*	1.08	3.61*
W(6) – indirect experience, second-order terms, selectivity adjusted	8.22*	–5.35*	3.27	–9.97*	–1.96	–6.75*
W(7) – direct experience, selectivity adjusted	1.44*	2.81*	1.30*	2.08*	3.04*	2.20*
W(8) – direct experience, second-order terms, selectivity adjusted	2.07*	2.45*	2.96	0.15	3.28*	2.16
Income elasticity						
Minimum	–0.16*	–0.58*	–0.19	0.08	0.07*	0.06
Maximum	–0.13*	–0.52*	–0.08	0.11*	0.09*	0.14*
Cross-wage elasticity						
Minimum	–0.18	–0.72	0.07	–0.04	–0.35	0.08
Maximum	0.22	–0.01	0.28	0.06	–0.04	0.26
Women in double-headed families:						
Wage measure						
Observed wage	1.22	...	...	0.97	...	...
W(1) – indirect experience, OLS	7.39	–0.40	–12.01	10.89	1.46	–2.10
W(2) – indirect experience, second-order terms, OLS	–11.46	4.97	–0.10	4.63	5.30*	–0.41
W(3) – direct experience, OLS	5.15*	5.21*	1.69	9.81*	10.14*	2.50
W(4) – direct experience, second-order terms, OLS	2.93	5.94*	2.44	–0.60	7.54*	3.63
W(5) – indirect experience, selectivity adjusted	1.58	–1.08	–11.18	14.10	2.60	–21.80
W(6) – indirect experience, second-order terms, selectivity adjusted	–18.49	0.13	–8.43	–4.63	3.88*	–0.72
W(7) – direct experience, selectivity adjusted	5.48*	3.94*	1.58	12.24*	7.85*	1.98
W(8) – direct experience, second-order terms, selectivity adjusted	5.02	5.05*	2.01	2.66	5.34*	1.36
Income elasticity						
Minimum	–0.02	–0.07	–0.02	0.17*	0.09	–0.01
Maximum	0.02	–0.05	0.01	0.18*	0.10	0.01
Cross-wage elasticity						
Minimum	–4.09	–1.57*	–1.22	–6.10*	–2.35	–0.73
Maximum	–1.35	1.12	0.48	–2.16	–1.07	0.82

Table 8-1 (concl'd.)

	Estimates of annual hours worked, without adjustment for taxes			Estimates of annual hours worked, with adjustment for taxes		
	OLS	Tobit model	Generalized Tobit model[2]	OLS	Tobit model	Generalized Tobit model[2]
Male single heads:						
Wage measure						
Observed wage	–0.43	. . .	. . .	–0.27	. . .	. . .
W(1) – indirect experience, OLS	3.40*	5.47*	5.54	15.76*	11.52*	11.84*
W(2) – indirect experience, second-order terms, OLS	3.96*	6.42*	6.57	–4.34	–5.95	–11.33*
W(3) – direct experience, OLS	2.66*	1.97	–3.64	4.18*	1.82	5.31
W(4) – direct experience, second-order terms, OLS	2.58*	2.48*	2.73*	5.52*	2.63	6.17
W(5) – indirect experience, selectivity adjusted	6.27*	22.05*	14.69	13.67*	9.62*	10.28*
W(6) – indirect experience, second-order terms, selectivity adjusted	4.60	23.55	15.11	–2.89	–5.05	–13.60
W(7) – direct experience, selectivity adjusted	4.52*	4.81	–6.33	10.99*	5.25	15.20
W(8) – direct experience, second-order terms, selectivity adjusted	4.76*	6.90	1.16	2.37	–2.40	–2.46
Income elasticity						
Minimum	0.33*	0.05	0.04	0.45	0.17*	–0.02
Maximum	0.38*	0.06*	0.29	0.50*	0.19*	0.36*
Female single heads:						
Wage measure						
Observed wage	0.17	. . .	. . .	0.15	. . .	. . .
W(1) – indirect experience, OLS	1.60	6.88*	27.30	13.39	13.23*	–0.18
W(2) – indirect experience, second-order terms, OLS	–1.19	–11.66*	–5.53	–2.51	–2.67	–0.83
W(3) – direct experience, OLS	–0.60	0.80	0.46	–4.57	–1.19	0.50
W(4) – direct experience, second-order terms, OLS	–0.95	–0.36	–0.08	–5.35*	–2.76	–0.09
W(5) – indirect experience, selectivity adjusted	1.53	5.23*	25.80	14.47	10.58*	–0.20
W(6) – indirect experience, second-order terms, selectivity adjusted	–0.57	–8.38	–0.83	–2.24	–1.73	–0.80
W(7) – direct experience, selectivity adjusted	–0.82	0.03	0.55	–4.44	0.21	0.68
W(8) – direct experience, second-order terms, selectivity adjusted	–0.76	0.37	0.16	–3.23	–0.42	–0.07
Income elasticity						
Minimum	–0.13*	–0.23*	–3.28	0.03	–0.10	–0.01
Maximum	0.12	–0.22*	–0.31	0.05	–0.09	0.00

*Denotes statistical significance at the 5-per-cent level.

1 Wage imputations are based on supplementary Table 1. Full results are given in supplementary Table 2. Both tables are available from the Economic Council of Canada upon request.

2 Corresponds to the selectivity model, equation 3.11.

is possible from the variety of estimation procedures used in previous studies even when the procedures are applied to the same database. As Mroz [1987] found, the specification of the wage equation and the method of handling non-workers affect the wage elasticity estimates in cross-sectional data. This conclusion holds whether taxes are incorporated in the model or not, at least so far as the linearized-budget-constraint treatment of taxes is concerned.

The upper left-hand corner of Table 8-1 for each group contains the least sophisticated estimates using OLS regression without adjustment for selection bias. The lower right-hand corner contains the most sophisticated estimates adjusting for selection bias and the effect of taxes. Looking across the table, we see the effect of the different estimation techniques for the same imputed wage measure. Looking down the table, we see the effect of using different imputed wages from the different wage equations in supplementary Table 1 for the same estimation technique for hours worked. The results suggest substantial variation in the uncompensated wage elasticity in both directions.

Like Mroz, we find considerable variation in the wage elasticity estimates for married women, but we see no pattern with regard to wage equation specification or estimation technique. Our conclusion based on the Mincome sample is that wage elasticity estimates are not robust in cross-sectional data. Unless cross-sectional studies establish that their estimates are robust at least in terms of specification, the results of these studies must be treated with considerable suspicion and caution.

Extending the analysis to other groups yields the same conclusion, however. Most of the recent attention has been devoted to married women because of the wide variation in empirical results and the assumption that problems such as sample selection and taxes are more important for this group. Hence Mroz's intensive econometric analysis of labour supply models is confined to married women. Our results for husbands and single heads in Table 8-1 do not suggest that the results for these groups are robust either. Thus the apparent agreement about labour supply results for men and single women may simply be the result of a lack of published research in these areas in recent years. Our results agree with the conclusion of DaVanzo et al. [1976] that male labour supply estimates are sensitive to the choice of statistical assumptions. We would note that the set of choices has widened since their work was completed.

We see no basis in the results in Table 8-1 for the almost exclusive attention to the labour supply of married women in recent years. The labour supply of married men and single household heads remain important subjects for investigation for the same reason as married women: results from cross-sectional data vary widely according to specification and estimation technique, providing little guidance for the analysis of policy alternatives. The likely reasons for this variation are frequently cited, that is, the presence of unobserved individual characteristics that affect labour supply and its determinants and the problems of selection bias and complex tax regimes. Mroz's findings for married women support these conclusions.

One solution is to proceed to test the statistical assumptions of each model that we estimate as Mroz has done. Another complementary approach is to use experimental data to avoid the problems inherent in cross-sectional data. We therefore turn to the experimental data from Mincome to provide new labour supply results that may be more accurate and robust than those available from prevailing cross-sectional evidence.

Structural Estimates from the Mincome Experimental Data

Experimental data can circumvent two of the three major problems associated with cross-sectional data, namely omitted individual characteristics and complex tax regimes. Since the third problem, the selection bias arising from the exclusion of non-workers, has been extensively studied in recent years, experimental data provide an excellent opportunity to deal effectively with each of these problems and to estimate labour supply response in their absence.

The problem of omitted individual characteristics can be addressed by using the within-groups estimator of equation 7.5 to eliminate individual fixed effects. Consider the conventional static labour supply model from equation 2.3 for the first adult:

$$h_1 = f_1 (w_1, w_2, y) \geq 0. \tag{2.3}$$

We can write the total differential for the labour supply function as

$$dh_1 = \frac{\partial h_1}{\partial w_1} dw_1 + \frac{\partial h_1}{\partial w_2} dw_2 + \frac{\partial h_1}{\partial y} dy, \tag{8.1}$$

and use the Slutsky equations 2.4 and 2.5

$$\frac{\partial h_i}{\partial w_i} = \frac{\partial f_i^s}{\partial w_i} + h_i \frac{\partial f_i}{\partial y} \qquad (i = 1, 2), \tag{2.4}$$

$$\frac{\partial h_i}{\partial w_j} = \frac{\partial f_i^s}{\partial w_j} + h_j \frac{\partial f_i}{\partial y} \qquad (i, j = 1, 2; i \neq j), \tag{2.5}$$

to obtain

$$dh_1 = \left(\frac{\partial f_1^s}{\partial w_1} + h_1 \frac{\partial f_1}{\partial y} \right) dw_1 + \left(\frac{\partial f_1^s}{\partial w_2} + h_2 \frac{\partial f_1}{\partial y} \right) dw_2 + \frac{\partial f_1}{\partial y} dy$$

$$= \left(\frac{\partial f_1^s}{\partial w_1}\right) dw_1 + \left(\frac{\partial f_1^s}{\partial w_2}\right) dw_2 + \frac{\partial f_1}{\partial y}\ (h_1 dw_1 + h_2 dw_2 + dy)$$

$$= c_1 dw_1 + c_2 dw_2 + c_3\ (h_1 dw_1 + h_2 dw_2 + dy). \qquad (8.2)$$

For single heads, $dw_2 = 0$. Keeley et al. [1978] and Keeley [1981, Chapter 2] suggest we convert the differential changes in equation 8.2 to finite differences between experimental years, which provides us with a version of equation 7.6:

$$Y_{it} - Y_i = (X_{it} - X_i)\,\beta + d\gamma + \xi_{it}, \qquad (7.6)$$

where

Y_{it} = hours worked in year t;
Y_i = the mean hours worked for all panels or, alternatively, some base year (the baseline hours worked);
X_{it} = wages, other income, and other control variables, such as children; and
d = a set of dummy variables for the fixed-time effects.

OLS regression can then be used to provide the within-groups estimates of the compensated wage effect, $\partial f_1^s/\partial w_1 = c_1$, the compensated cross-wage effect, $\partial f_1^s/\partial w_2 = c_2$, and the income effect, $h_1(\partial f_1/\partial y) = h_1 c_3$. The change in family income, $h_1 dw_1 + h_2 dw_2 + dy$ in equation 8.2, is determined on the basis of initial labour supply for the husband and wife (h_1 and h_2 from the base year). Equations 2.7, 2.8, and 2.9 can then be used to compute the corresponding elasticity estimates that, because they are derived from the within-groups estimator, are not biased by individual-specific fixed effects.

The problem of adjusting for taxes is simplified by the presence of a single flat-rate tax change induced experimentally. Provided that the tax regime remains unchanged otherwise, and provided that the structure of wages is stable (a matter we consider below), the only variation in after-tax wages arises in the experimental group. Since this tax rate change generates a single marginal tax rate for each household, problems of bias arising from the dependence of the tax rate on hours worked do not arise.

It might be noted that these conditions did not prevail in the two major income maintenance experiments in the United States. In the New Jersey experiment, there were changes in the tax-transfer system during the experiment. In the Seattle-Denver experiment, a declining marginal tax rate was applied to part of the sample. In each case, the effect of these conditions was not carefully investigated as far as we know.

The model proposed by equations 8.2 and 7.6 can be estimated provided that there is sufficient variation in after-tax wage rates and other income over time for the individuals in the sample. This is unlikely in non-experimental longitudinal data unless there is a significant change in the tax-transfer system, which is rare, or a long time period is considered. When a long time period under a stable tax-transfer system is considered, much of the change in wages and unearned income will be anticipated life-cycle effects. The proper estimation of these equations requires dramatic short-term changes in taxes and transfers of the sort that an income maintenance experiment such as Mincome provides.

Table 8-2 indicates the dramatic short-term changes in the crucial labour supply determinants in the Mincome data for four groups – men and women in single- and double-headed families – between 1974 and 1976. The middle experimental year is chosen because it is likely to be the indicator of labour supply response that is least contaminated by any "start up" or "wind down" effect of the experiment [Keeley et al., 1978]. For the control group, nominal after-tax wages rise similarly for all groups, from 30 per cent for single women to 38 per cent for married men. For the experimental group, however, after-tax wages are *reduced* by between 3 per cent for single women and 24 per cent for married women. For the control group, virtual income *falls* by between 16 per cent for married women and 41 per cent for single women, but it *rises* by between 124 per cent for married men and 3,339 per cent for married women in the experimental group. These dramatic differences in after-tax wages and virtual income between the experimental and control groups should help to obtain precise estimates of the crucial labour supply parameters.

As for the baseline data analysis, we calculate eight wage equations that vary according to specification (indirect or direct experience measure, and presence or absence of second-order terms for education and age) and estimation method (OLS versus adjustment for selection bias). The results are presented in Table 8-3. In this case the wage equations are estimated over all four years (1974-1977) and dummy variables are included to capture time effects. Otherwise the specification is identical to that in supplementary Table 1.

The results in Table 8-3 do not differ greatly from those in supplementary Table 1. The coefficient for the selection

Table 8-2

Mean Values for Critical Labour Supply Variables from the Mincome Data for the Experimental and Control Groups, 1974-76[1]

	Experimental group			Control group		
	Panel 1	Panel 3	Percentage change	Panel 1	Panel 3	Percentage change
Variables:						
Men in double-headed families						
After-tax wage	$2.83	$2.56	-9.5	$2.71	$3.75	38.2
Virtual income	$2,284.30	$5,115.63	123.9	$2,293.54	$1,453.09	-36.6
Hours worked	1,819.3	1,568.0	-13.8	1,842.1	1,687.3	-8.4
Women in double-headed families						
After-tax wage	$2.15	$1.62	-24.4	$2.22	$2.98	34.4
Virtual income	$148.76	$5,115.63	3,338.6	$171.76	$144.59	-15.8
Hours worked	521.9	433.4	-17.0	558.8	587.0	5.1
Single male heads						
After-tax wage	$2.77	$2.39	-13.7	$2.54	$3.43	-34.9
Virtual income	$1,042.05	$2,549.57	144.7	$997.08	$624.04	-37.4
Hours worked	1,271.1	1,085.9	-14.6	1,250.6	945.1	-24.4
Single female heads						
After-tax wage	$1.89	$1.84	-2.8	$1.98	$2.58	30.3
Virtual income	$491.45	$2,549.57	418.8	$340.56	$199.64	-41.4
Hours worked	1,362.39	1,059.89	-22.2	1,088.26	873.03	-19.8

[1] Pre-experimental panel 1, 1974, versus panel 3, 1976.

bias adjustment term, λ, is always significant, indicating that the OLS estimates are biased. A Chow-test for the stability of the wage equations over time was performed for each specification. For the OLS equations, the low *F*-values indicate that the hypothesis of a stable wage structure over time cannot be rejected. This is a fortunate result because it suggests that after-tax wage changes over time are primarily attributable to tax changes and not changes in before-tax behaviour. The equivalent Chow-test results are also shown for the wage equations adjusted for selection bias. The *F*-values are also low, as we would hope, but the *F*-test is not valid for these results because the estimation procedure is known to generate heteroscedastic errors [Heckman, 1979]. The results suggest, however, that the wage equations are stable as desired.

To estimate the labour supply equations we use the wage equations that correct for sample selection bias, since λ is significant in every case in Table 8-3. The imputed wages from each of these equations are used to estimate four structural labour supply models from the panel data:

1 A strict version of equations 8.2 and 7.6 given by:

$$Y_{it} - Y_i = (X_{it} - X_i)\,\beta_1 + d\gamma_1 + \xi^1_{it} \quad (t = 1, 2, 3, 4), \qquad (8.3)$$

where Y_i is the mean hours worked by the individual in the four panels and X_i is the mean over the four panels.

2 A similar equation given by:

$$Y_{it} - Y_{i1} = (X_{it} - X_{i1})\,\beta_2 + d\gamma_2 + \xi^2_{it} \quad (t = 2, 3, 4), \qquad (8.4)$$

where Y_{i1} and X_{i1} refer to the baseline survey (1974).

3 A version of equation 8.4 restricted to comparisons between the baseline and the mid-year of the experiment (1976):

$$Y_{i3} - Y_{i1} = (X_{i3} - X_{i1})\,\beta_3 + \xi^3_i. \qquad (8.5)$$

Keeley et al. [1978], for example, argue that the strongest, and most accurate, labour supply response will occur

Table 8-3

Wage Equations for Men and Women, Based on Mincome Data, 1974-77^a

	OLS				Selection bias adjusted^b			
	$W(1)$	$W(2)$	$W(3)$	$W(4)$	$W(5)$	$W(6)$	$W(7)$	$W(8)$
Men:								
Variables								
Intercept	2.724	2.555	3.574	4.941	2.972	2.935	3.379	4.887
	(11.06)	(2.86)	(13.22)	(4.25)	(11.83)	(3.29)	(12.56)	(4.24)
Education	0.038	0.067	0.034	–0.174	0.049	0.034	0.042	–0.147
	(2.38)	(0.52)	(2.35)	(1.75)	(3.06)	(0.27)	(2.89)	(1.51)
Education^2	. . .	–0.001	. . .	–0.002	. . .	0.0014	. . .	–0.0013
	. . .	(0.29)	. . .	(0.65)	. . .	(0.47)	. . .	(0.42)
Direct experience^c	. . .	. . .	0.088	0.056	. . .	. . .	0.066	0.047
	. . .	. . .	(6.02)	(2.92)	. . .	. . .	(4.36)	(2.40)
Direct experience^2	. . .	. . .	–0.002	–0.001	. . .	. . .	–0.0016	–0.001
	. . .	. . .	(4.92)	(1.75)	. . .	. . .	(4.43)	(1.97)
Indirect experience^d	0.097	0.101	. . .	. . .	0.076	0.085	. . .	. . .
	(7.24)	(2.84)	. . .	. . .	(5.26)	(2.39)	. . .	. . .
Indirect experience^2	–0.002	–0.002	. . .	. . .	–0.0015	–0.0016	. . .	. . .
	(6.83)	(4.93)	. . .	. . .	(4.68)	(3.88)	. . .	. . .
Age	. . .	. . .	–0.017	–0.023	. . .	. . .	–0.0013	–0.026
	. . .	. . .	(2.18)	(0.46)	. . .	. . .	(0.15)	(0.52)
Age^2	. . .	. . .	. . .	–0.001	. . .	. . .	. . .	–0.0004
	. . .	. . .	. . .	(1.33)	. . .	. . .	. . .	(0.73)
Age/education	. . .	–0.0003	. . .	0.007	. . .	–0.0003	. . .	0.006
	. . .	(0.12)	. . .	(3.88)	. . .	(0.13)	. . .	(3.37)
Panel 2 (1975)	0.451	0.451	0.496	0.490	0.588	0.578	0.613	0.594
	(4.36)	(4.35)	(4.73)	(4.73)	(5.50)	(5.42)	(5.82)	(5.67)
Panel 3 (1976)	1.370	1.370	1.389	1.373	1.600	1.587	1.584	1.550
	(13.13)	(13.11)	(13.22)	(13.19)	(14.09)	(14.05)	(14.46)	(14.17)
Panel 4 (1977)	2.198	2.197	2.143	2.133	2.429	2.414	2.358	2.323
	(20.62)	(20.58)	(19.96)	(20.08)	(20.58)	(20.61)	(20.68)	(20.40)
λ^b	. . .	. . .	. . .	. . .	–0.498	–0.481	–0.539	–0.466
	. . .	. . .	. . .	. . .	(4.25)	(4.16)	(4.70)	(4.03)
$\bar{R}^2$	0.434	0.434	0.432	0.449	0.454	0.454	0.457	0.468
F	105.74	79.13	80.20	59.80	95.58	74.04	75.82	57.40
n	834	834	745	745	812	812	731	731
F (interaction terms)^e	1.38	1.02	0.73	0.49	2.58	2.15	1.94	1.63
Women:								
Variables								
Intercept	1.492	2.164	1.827	1.616	1.752	2.302	2.018	1.539
	(8.36)	(3.32)	(9.42)	(1.70)	(8.24)	(3.37)	(9.50)	(1.57)
Education	0.067	–0.113	0.077	–0.131	0.070	–0.091	0.084	–0.113
	(5.33)	(1.12)	(5.92)	(1.24)	(5.38)	(0.87)	(6.20)	(1.04)

Table 8-3 (concl'd.)

	OLS				Selection bias adjusted[b]			
	W(1)	*W*(2)	*W*(3)	*W*(4)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Education^2	. . .	0.011	. . .	0.012	. . .	0.001	. . .	0.012
	. . .	(3.68)	. . .	(3.32)	. . .	(3.29)	. . .	(3.13)
Direct experience^c	. . .	. . .	0.034	0.029	. . .	. . .	0.032	0.025
	. . .	. . .	(2.65)	(2.09)	. . .	. . .	(2.41)	(1.69)
Direct experience^2	. . .	. . .	–0.001	–0.0007	. . .	. . .	–0.0012	–0.0008
	. . .	. . .	(2.02)	(1.24)	. . .	. . .	(2.34)	(1.36)
Indirect experience^d	0.036	0.047	. . .	. . .	0.037	0.049	. . .	. . .
	(3.96)	(1.97)	. . .	. . .	(3.92)	(1.95)	. . .	. . .
Indirect experience^2	–0.001	–0.001	. . .	. . .	–0.001	–0.001	. . .	. . .
	(4.66)	(4.09)	. . .	. . .	(4.66)	(4.06)	. . .	. . .
Age	. . .	. . .	–0.010	0.057	. . .	. . .	–0.0078	0.70
	. . .	. . .	(2.82)	(1.87)	. . .	. . .	(2.01)	(2.21)
Age^2	. . .	. . .	. . .	–0.0008	. . .	–0.0006	. . .	–0.001
	. . .	. . .	. . .	(2.82)	. . .	. . .	. . .	(3.14)
Age/education	. . .	–0.0005	. . .	–0.0008	. . .	–0.0006	. . .	–0.0008
	. . .	(0.35)	. . .	(0.50)	. . .	(0.36)	. . .	(0.51)
Panel 2 (1975)	0.315	0.308	0.364	0.329	0.394	0.379	0.465	0.417
	(4.29)	(4.23)	(4.65)	(4.26)	(4.59)	(4.46)	(4.91)	(4.50)
Panel 3 (1976)	1.141	1.135	1.139	1.114	1.258	1.241	1.285	1.241
	(14.69)	(14.75)	(13.62)	(13.51)	(12.91)	(12.84)	(11.69)	(11.57)
Panel 4 (1977)	1.502	1.503	1.581	1.554	1.638	1.625	1.739	1.688
	(19.00)	(19.13)	(18.71)	(18.46)	(15.86)	(15.83)	(15.28)	(15.16)
λ^b	. . .	. . .	. . .	. . .	–0.273	–0.243	–0.305	–0.268
	. . .	. . .	. . .	. . .	(2.42)	(2.18)	(2.40)	(2.17)
R^2	0.476	0.489	0.482	0.507	0.470	0.481	0.476	0.502
F	92.05	72.57	69.27	53.42	72.92	59.22	56.27	45.02
n	616	616	530	530	584	584	504	504
F (interaction terms)^e	1.04	1.47	1.90	1.66	2.00	2.40	2.77	2.38

a Data are from all panels. The dependent variable is hourly rated wages. Figures in parentheses are *t*-values.

b Adjusted using Heckman's [1979] procedure with Greene's [1981] correction for heteroscedasticity from the LIMDEP software package. The probit equation for participation includes the variables in the corresponding wage equation plus other household income, number of preschool children, and marginal tax rate. The measure of experience is the same as that appearing in the corresponding wage equation. Other household income is virtual income, calculated by linearizing the family budget constraint at the prevailing marginal tax rates of the husband and wife.

c Years in the labour force since leaving school.

d Age minus years of schooling minus 5.

e Tests the hypothesis of stable slope coefficients over the duration of the experiment. The standard *F*-test is not valid when the wage equation is adjusted for selection bias using Heckman's procedure because the errors are heteroscedastic.

in the mid-year of the experiment because of "start up" and "wind down" effects. Thus the evidence from the other two experimental panels may bias the estimates of labour supply response.

4 A version of equation 8.5 by Keeley et al. [1977, 1978] given by:

$$Y_{i3} = (X_{i3} - X_{i1})\,\beta_4 + cY_{i1} + \xi_i^4 \tag{8.6}$$

Keeley et al. argue that equation 8.5 will generate biased estimates because the change in virtual income is measured using actual pre-experimental labour supply rather than permanent labour supply. This bias may be corrected by the inclusion of the pre-experimental labour supply, Y_{i1}, as an explanatory variable. They then estimate equation 8.6 by Tobit regression.

The approach by Keeley et al. has been quite influential both as a method of estimating structural labour supply models from experimental data and as a source of evidence on the labour supply behaviour of low-income households. For this reason, we have included their approach in this study and used their model extensively to assess labour supply response based on the Mincome data in an earlier paper [Simpson et al., 1987]. In that paper we found evidence of a stronger response in the middle year, justifying equations 8.5 and 8.6.

We are uncomfortable with the approach taken by Keeley et al. for several reasons, however. First, they do not provide a rigorous justification for their reformulation of the basic model given by equation 8.2. As a crude adjustment for the errors-in-variables problem concerning virtual income, the inclusion of pre-experimental labour supply as a control variable may have other unintended effects. Our earlier research suggests that the results from Mincome using their model are quite sensitive to the inclusion of pre-experimental labour supply and, indeed, that this variable dominates in terms of explanatory power. Thus any unintended effect arising from the inclusion of this variable could be quite serious. Second, there is no justification for the use of the Tobit model. Pre-experimental labour supply could simply have been included as a control variable in equation 8.5 to meet their concerns. Since the dependent variable in equation 8.5 (or 8.3 or 8.4) is the difference between two limited variables ($Y_{i3} \geq 0$, $Y_{i1} \geq 0$), specification of the distribution of the error term and a consistent estimation technique may be difficult. It seems to us, however, that ignoring this problem and using OLS in equations 8.3, 8.4, and 8.5 is a less serious error than using Tobit regression in equation 8.6, particularly in view of Mroz's [1987] rejection of the Tobit estimator for labour supply models. Hence we include the model by Keeley et al. for the sake of comparison and not without serious reservations.

For purposes of comparison, a standard set of regressors is used for each of the four models for all labour force groups:

1 The critical labour supply variables discussed above, namely:

DWM = the change in after-tax wages (for a male);
DWF = the change in after-tax wages (for a female); and
DV = the change in virtual income.

For males (females), *DWF* (*DWM*) refers to the wage of the spouse, if present.

2 The fixed-time effects, namely:

PANEL 2 = 1 only for 1975 (the first experimental year),
= 0 otherwise;
PANEL 3 = 1 only for 1976 (the second experimental year),
= 0 otherwise; and
PANEL 4 = 1 only for 1977 (the final experimental year),
= 0 otherwise.

3 Changes in household circumstances, namely:

DCHLD = the change in the number of preschool children.

4 Variables to correct for experimental assignment problems, as discussed in Chapter 7, namely:

NIC_i = 1, if household falls into normal income cell i (i = 2, 3, 4, 5),
= 0 otherwise;
BREAK = the break-even level of assigned plan;
FABOVE = 1, if household earnings is above *BREAK* at the baseline;
= 0 otherwise; and
EARNABV = earnings minus *BREAK*, if *FABOVE* = 1.

These variables are all defined at the baseline.

5 Other control variables as in Keeley et al. [1978], namely:

BASEHOURS = pre-experimental labour supply for reasons discussed above;
AGE = age at the baseline survey;
FAMSIZE = family size at the baseline survey;
CHILDREN = number of preschool children at the baseline survey; and
WELFARE = the amount of social assistance received by the family in the baseline year (1974).

The variables are included to facilitate comparison with the results from Keeley et al.

The four labour supply models are estimated for each of the four wage rate measures, corresponding to columns 5 to 8 of Table 8-3, for married men, married women, single male heads, and single female heads. The results are presented in Tables 8-4 to 8-7. One noticeable surprise is that the compensated wage effect (the coefficient of *DWM* for males and *DWF* for females) is often negative, particularly for women for all estimates but also for men for equation 8.3. In other respects the results for equation 8.3 are quite appealing. The wage and income effects are generally significant and account for much more of the explanatory power than in the other equations. In fact, the control variables, including the variables to correct for experimental

Table 8-4

Estimates of Labour Supply Equations, Based on Mincome Data, Male Single Heads, 1974-77[1]

	Equation 8.3 (change in hours worked from average of four panels)				Equation 8.4 (change in hours worked from the baseline)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	92.27	72.14	187.34	189.94	877.05	867.77	1,000.00	988.43
	(0.50)	(0.38)	(0.48)	(0.49)	(3.87)	(3.87)	(2.70)	(2.66)
PANEL 2	–211.45	–187.72	–229.88	–237.49	...	...	...	...
	(1.95)	(1.74)	(1.67)	(1.72)	...	...	...	...
PANEL 3	–107.5	–80.73	–265.8	–267.93	–101.84	–103.75	–266.57	–264.44
	(0.96)	(0.71)	(1.90)	(1.89)	(0.90)	(0.91)	(1.89)	(1.87)
PANEL 4	–50.13	–20.10	–253.68	–254.33	28.18	25.81	–192.28	–191.00
	(0.41)	(0.16)	(1.61)	(1.59)	(0.22)	(0.20)	(1.23)	(1.22)
DWM	–87.64	–66.15	4.09	–4.03	–17.22	–5.90	44.75	39.10
	(1.41)	(0.98)	(0.05)	(0.05)	(0.35)	(0.11)	(0.72)	(0.62)
DV	0.05	0.03	0.063	0.063	0.06	0.06	0.12	0.12
	(1.10)	(0.69)	(1.11)	(1.08)	(1.84)	(1.64)	(2.97)	(2.95)
DCHLD	711.28	709.39	658.63	674.46	290.73	282.44	2,007.90	2,022.60
	(1.79)	(1.78)	(1.08)	(1.11)	(0.54)	(0.53)	(2.30)	(2.30)
BASEHOURS	0.00	0.00	0.00	0.00	–0.74	–0.75	–0.73	–0.73
	(0.00)	(0.00)	(0.00)	(0.00)	(11.82)	(12.07)	(8.47)	(8.43)
NIC2	...	...	...	...	15.79	18.27	137.66	137.43
	...	...	...	...	(0.10)	(0.12)	(0.61)	(0.61)
NIC3	...	...	...	...	97.04	101.17	160.50	154.00
	...	...	...	...	(0.65)	(0.67)	(0.73)	(0.70)
NIC4	...	...	...	...	208.35	210.59	183.90	185.51
	...	...	...	...	(1.15)	(1.16)	(0.74)	(0.75)
NIC5	...	...	...	...	279.40	273.79	–1,330.60	–1,326.13
	...	...	...	...	(0.50)	(0.49)	(1.80)	(1.78)
EARNABV	...	...	...	...	0.20	0.19	1.10	1.09
	...	...	...	...	(1.07)	(1.05)	(3.10)	(3.05)
FABOVE	...	...	...	...	–668.88	–640.04	–5,614.60	–5,566.70
	...	...	...	...	(0.53)	(0.50)	(2.70)	(2.67)
BREAK	...	...	...	...	0.04	0.04	0.08	0.08
	...	...	...	...	(1.67)	(1.68)	(3.02)	(2.98)
AGE	...	...	...	...	–11.03	–10.89	–13.57	–13.44
	...	...	...	...	(2.01)	(1.97)	(1.96)	(1.94)
FAMSIZE	...	...	...	...	46.87	50.05	–29.89	–30.30
	...	...	...	...	(0.77)	(0.82)	(0.45)	(0.45)
CHILDREN	...	...	...	...	110.43	117.65	1,173.10	1,186.00
	...	...	...	...	(0.23)	(0.25)	(2.07)	(2.08)
WELFARE	...	...	...	...	–0.48	–0.49	–0.53	–0.52
	...	...	...	...	(2.21)	(2.26)	(2.36)	(2.30)
R^2	0.041	0.035	0.052	0.05	0.46	0.46	0.57	0.56
F	0.52	0.44	0.38	0.37	11.47	11.40	9.38	9.29
n	224	224	128	128	249	249	141	141

Table 8-4 (concl'd.)

	Equation 8.5 (change in hours worked in 1976 versus the 1974 baseline)				Equation 8.6 (hours worked in 1976)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	1,286.40	1,297.20	678.51	671.88	1,402.10	1,320.30	420.24	73.38
	(3.21)	(3.22)	(0.93)	(0.91)	(3.28)	(3.31)	(0.56)	(0.29)
PANEL 2	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
PANEL 3	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
PANEL 4	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
	. . .	. . .	. . .	. . .	. . .	. . .	. . .	. . .
DWM	1.37	4.79	–8.77	–20.82	–33.53	–29.15	–104.20	–174.58
	(0.02)	(0.05)	(0.09)	(0.20)	(0.36)	(0.29)	(0.74)	(1.47)
DV	0.03	0.02	0.19	0.19	0.05	–0.04	0.24	0.23
	(0.47)	(0.29)	(2.26)	(2.24)	(0.70)	(0.48)	(2.51)	(2.81)
DCHILD	–1,988.00	–1,999.50	2,127.00	2,161.40	–2,104.60	–2,133.60	36,783.00	1,635.40
	(1.66)	(1.66)	(1.49)	(1.52)	(1.71)	(1.71)	(0.00)	(2.74)
BASEHOURS	–0.86	–0.86	–0.70	–0.70	0.15	0.14	0.39	0.47
	(7.52)	(7.56)	(4.01)	(3.99)	(1.26)	(1.22)	(2.09)	(3.27)
NIC2	–213.80	–213.22	205.25	199.93	–272.35	–266.20	243.91	. . .
	(0.78)	(0.78)	(0.50)	(0.49)	(0.92)	(0.89)	(0.54)	. . .
NIC3	–33.67	–31.62	–43.89	–50.43	31.64	36.87	42.28	. . .
	(0.13)	(0.12)	(0.11)	(0.13)	(0.11)	(0.13)	(0.10)	. . .
NIC4	–107.33	–105.29	–22.76	–19.16	–78.29	–72.50	41.42	. . .
	(0.32)	(0.31)	(0.05)	(0.04)	(0.23)	(0.21)	(0.09)	. . .
NIC5	4,920.30	4,939.60	–55.15	–22.81	5,335.90	5,363.80	–54,506.00	. . .
	(2.47)	(2.47)	(0.06)	(0.03)	(2.56)	(2.56)	(0.00)	. . .
EARNABV	–0.42	–0.43	–0.47	–0.47	–0.43	–0.44	–202.69	. . .
	(1.29)	(1.31)	(1.00)	(1.04)	(1.30)	(1.32)	(0.00)	. . .
FABOVE	4,304.00	4,354.10	. . .	. . .	4,403.00	4,465.80	122,980.00	. . .
	(1.72)	(1.73)	. . .	. . .	(1.72)	(1.74)	(0.00)	. . .
BREAK	0.00	0.00	0.06	0.06	0.02	0.02	0.11	. . .
	(0.00)	(0.04)	(0.98)	(0.93)	(0.45)	(0.40)	(1.74)	. . .
AGE	–21.57	–21.75	–13.41	–13.33	–36.09	–36.37	–22.97	. . .
	(2.22)	(2.23)	(1.03)	(1.02)	(3.21)	(3.22)	(1.72)	. . .
FAMSIZE	218.48	225.82	0.07	2.11	295.59	303.62	0.00	. . .
	(1.79)	(1.84)	(0.00)	(0.01)	(2.30)	(2.35)	(0.00)	. . .
CHILDREN	–4,604.00	–4,594.60	. . .	. . .	–5,138.60	–5,128.90	42,159.00	. . .
	(2.62)	(2.61)	. . .	. . .	(2.80)	(2.79)	(0.00)	. . .
WELFARE	–0.49	–0.50	–0.21	–0.19	–0.70	–0.71	–0.26	. . .
	(1.24)	(1.25)	(0.49)	(0.44)	(1.66)	(1.67)	(0.56)	. . .
R^2	0.56	0.56	0.66	0.66	. . .	. . .	. . .	. . .
F	5.30	5.28	4.16	4.11	. . .	. . .	. . .	. . .
n	79	79	42	42	79	79	42	42

[1] Regression technique is OLS for columns 1 to 3 of each panel. The Tobit model was used for column 4 of each panel. Figures in parentheses are *t*-values. Imputed wages are based on columns 5 to 8 of Table 8-3.

Table 8-5

Estimates of Labour Supply Equations, Based on Mincome Data, Female Single Heads, 1974-77[1]

	Equation 8.3 (change in hours worked from average of four panels)				Equation 8.4 (change in hours worked from the baseline)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	47.37	-62.09	91.01	182.89	892.02	911.67	617.58	611.43
	(0.50)	(0.63)	(0.70)	(1.40)	(7.31)	(7.55)	(3.89)	(3.17)
PANEL 2	-86.56	14.79	-119.10	-106.16	...	...	...	...
	(1.37)	(0.23)	(1.73)	(1.47)	...	...	...	...
PANEL 3	-80.26	76.66	-135.15	-263.54	-101.50	-62.67	-133.00	-187.45
	(1.17)	(0.98)	(1.80)	(3.09)	(1.56)	(0.92)	(1.87)	(2.61)
PANEL 4	-22.68	156.90	-109.79	-361.86	-58.43	1.32	-103.92	-202.16
	(0.30)	(1.72)	(1.30)	(3.82)	(0.79)	(0.02)	(1.28)	(2.49)
DWF	-90.07	-139.47	-82.37	313.30	-77.93	-113.84	-39.25	103.01
	(2.36)	(2.71)	(2.05)	(4.80)	(2.30)	(2.54)	(1.06)	(1.54)
DV	-0.04	-0.09	-0.04	-0.10	0.03	0.00	0.00	-0.02
	(1.60)	(3.47)	(1.45)	(4.60)	(1.23)	(0.05)	(0.03)	(0.85)
DCHILD	-125.33	-138.87	-178.29	-171.67	-277.20	-273.28	-292.76	-289.33
	(1.80)	(2.01)	(2.40)	2.39	(3.65)	(3.60)	(3.32)	(3.27)
BASEHOURS	0.00	0.00	0.00	0.00	-0.54	-0.55	-0.53	-0.50
	(0.00)	(0.00)	(0.00)	(0.00)	(16.22)	(16.75)	(14.17)	(11.13)
NIC2	...	...	...	...	29.95	33.33	212.01	222.08
	...	...	...	...	(0.31)	(0.35)	(1.65)	(1.75)
NIC3	...	...	...	...	55.02	62.13	248.72	267.61
	...	...	...	...	(0.57)	(0.64)	(1.93)	(2.09)
NIC4	...	...	...	...	266.35	276.60	425.57	435.96
	...	...	...	...	(2.47)	(2.57)	(3.12)	(3.21)
NIC5	...	...	...	...	-22.46	-29.04	58.90	34.73
	...	...	...	...	(0.07)	(0.10)	(0.15)	(0.14)
EARNABV	...	...	...	...	-0.02	-0.03	-0.02	-0.01
	...	...	...	...	(0.27)	(0.45)	(0.27)	(0.09)
FABOVE	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
BREAK	...	...	...	...	-0.02	-0.01	0.00	0.01
	...	...	...	...	(1.68)	(0.95)	(0.28)	(0.43)
AGE	...	...	...	...	-7.68	-7.94	-5.01	-7.75
	...	...	...	...	(3.05)	(3.16)	(1.83)	(2.29)
FAMSIZE	...	...	...	...	4.21	8.65	-8.84	-7,032.00
	...	...	...	...	(0.16)	(0.33)	(0.32)	(0.27)
CHILDREN	...	...	...	...	-282.95	-277.12	-234.82	-225.67
	...	...	...	...	(4.24)	(4.15)	(3.20)	(3.09)
WELFARE	...	...	...	...	-0.19	-0.18	-0.06	-0.05
	...	...	...	...	(2.61)	(2.59)	(0.73)	(0.60)
R^2	0.04	0.05	0.06	0.10	0.40	0.40	0.36	0.37
F	1.19	1.69	1.73	2.97	22.67	22.77	16.29	16.40
n	568	568	468	468	570	570	474	474

Table 8-5 (concl'd.)

	Equation 8.5 (change in hours worked in 1976 versus the 1974 baseline)				Equation 8.6 (hours worked in 1976)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	748.30	806.67	318.24	377.29	–268.15	185.15	46.96	164.27
	(3.68)	(3.94)	(1.10)	(1.22)	(1.54)	(1.54)	(0.31)	(0.94)
PANEL 2	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
PANEL 3	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
PANEL 4	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
DWF	–119.49	–144.16	–66.21	–113.06	–276.78	–216.43	–115.64	–73.04
	(2.13)	(1.92)	(1.02)	(0.93)	(3.63)	(3.63)	(1.63)	(0.61)
DV	0.10	0.08	0.05	0.04	0.14	0.14	0.02	0.02
	(2.16)	(1.51)	(0.98)	(0.80)	(2.43)	(2.43)	(0.50)	(0.43)
DCHLD	–319.30	–321.67	–359.87	–385.88	57.64	29.40	86.48	49.39
	(2.54)	(2.55)	(2.25)	(2.41)	(0.44)	(0.22)	(0.60)	(0.35)
BASEHOURS	–0.56	–0.59	–0.58	–0.64	0.61	0.68	0.68	0.64
	(10.13)	(10.70)	(8.91)	(7.44)	(7.78)	(9.61)	(8.61)	(6.76)
NIC2	22.45	34.84	358.93	391.74	589.99	...	...	...
	(0.13)	(0.21)	(1.46)	(1.64)	(3.01)	...	...	...
NIC3	–43.42	–25.84	313.65	341.64	543.34	...	...	...
	(0.25)	(0.15)	(1.27)	(1.40)	(2.69)	...	...	...
NIC4	165.51	185.40	503.24	520.77	836.10	...	...	...
	(0.87)	(0.97)	(1.93)	(2.02)	(3.77)	...	...	...
NIC5	130.25	134.06	297.62	253.88	397.04	...	...	...
	(0.27)	(0.28)	(0.44)	(0.37)	(0.60)	...	...	...
EARNABV	–0.01	–0.02	–0.01	–0.02	–4.83	...	...	...
	(0.14)	(0.25)	(0.12)	(0.18)	(0.00)	...	...	...
FABOVE	...	...	...	...	0.24	...	...	...
	...	...	...	...	(0.00)	...	...	...
BREAK	–0.04	–0.04	–0.02	0.00	–0.08	...	...	...
	(1.82)	(1.55)	(0.68)	(0.19)	(2.91)	...	...	...
AGE	–6.38	–6.71	–3.41	–0.94	...	...	...	...
	(1.46)	(1.53)	(0.69)	(0.15)	...	...	...	...
FAMSIZE	41.66	43.05	29.26	20.06	...	...	...	...
	(0.96)	(0.98)	(0.62)	(0.43)	...	...	...	...
CHILDREN	–424.78	–423.99	–388.64	–377.07	...	...	...	...
	(3.79)	(3.77)	(2.97)	(2.86)	...	...	...	...
WELFARE	–0.25	–0.25	–0.06	–0.06	...	...	...	...
	(2.00)	(1.97)	(0.38)	(0.39)	...	...	...	...
R^2	0.48	0.47	0.45	0.45	...	...	...	...
F	10.83	10.65	7.73	7.65	...	...	...	...
n	181	181	148	148	181	181	148	148

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1 Regression technique is OLS for columns 1 to 3 of each panel. The Tobit model was used for column 4 of each panel. Figures in parentheses are *t*-values. Imputed wages are based on columns 5 to 8 of Table 8-3.

Table 8-6

Estimates of Labour Supply Equations, Based on Mincome Data, Men in Double-Headed Families, 1974-77[1]

	Equation 8.3 (change in hours worked from average of four panels)				Equation 8.4 (change in hours worked from the baseline)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	48.61	87.67	64.19	72.86	490.12	475.86	651.38	642.96
	(0.55)	(0.99)	(0.65)	(0.66)	(3.74)	(3.61)	(4.27)	(4.19)
PANEL 2	–27.90	–59.74	–58.34	–91.40	...	...	...	...
	(0.64)	(1.44)	(1.23)	(1.53)	...	...	...	...
PANEL 3	–103.38	–159.84	–107.43	–107.24	–58.66	–72.29	–69.41	–39.66
	(2.34)	(3.54)	(2.22)	(1.28)	(1.37)	(1.39)	(1.27)	(0.66)
PANEL 4	–63.15	–131.10	–90.98	–92.79	–55.24	–63.11	–68.76	–20.00
	(1.20)	(2.48)	(1.62)	(0.92)	(0.99)	(1.09)	(1.14)	(0.27)
DWM	–163.70	–158.68	–154.25	–38.94	28.63	41.44	105.32	89.73
	(4.34)	(3.88)	(3.61)	(1.45)	(0.83)	(1.12)	(2.67)	(3.89)
DWF	223.35	266.17	194.76	–28.51	49.11	45.64	–6.13	–41.50
	(4.75)	(4.25)	(3.76)	(0.49)	(1.02)	(0.70)	(0.12)	(0.80)
DV	–0.02	–0.02	–0.02	–0.01	–0.02	–0.02	–0.03	–0.04
	(2.18)	(2.35)	(1.77)	(1.02)	(2.42)	(2.47)	(3.42)	(3.95)
DCHLD	76.94	74.16	99.36	105.02	125.09	125.23	144.12	139.50
	(2.58)	(2.47)	(2.90)	(3.03)	(3.97)	(3.98)	(4.03)	(3.91)
BASEHOURS	0.00	0.00	0.00	0.00	–0.62	–0.62	–0.68	–0.69
	(0.00)	(0.00)	(0.00)	(0.00)	(21.91)	(22.07)	(21.42)	(21.92)
NIC2	...	...	...	...	231.31	229.98	189.84	170.19
	...	...	...	...	(2.35)	(2.34)	(1.72)	(1.54)
NIC3	...	...	...	...	322.09	320.81	298.37	294.70
	...	...	...	...	(3.46)	(3.45)	(2.91)	(2.87)
NIC4	...	...	...	...	473.16	472.28	441.83	437.99
	...	...	...	...	(5.53)	(5.34)	(4.56)	(4.53)
NIC5	...	...	...	...	463.48	461.63	445.86	444.52
	...	...	...	...	(5.03)	(5.01)	(4.47)	(4.47)
EARNABV	...	...	...	...	–0.04	–0.04	–0.03	-0.03
	...	...	...	...	(2.24)	(2.14)	(1.62)	(1.68)
FABOVE	...	...	...	...	359.41	361.77	354.88	401.48
	...	...	...	...	(3.73)	(3.77)	(3.36)	(3.86)
BREAK	...	...	...	...	–0.01	–0.01	–0.01	–0.01
	...	...	...	...	(2.00)	(2.12)	(2.26)	(2.31)
AGE	...	...	...	...	2.21	2.36	2.04	3.50
	...	...	...	...	(0.79)	(0.84)	(0.66)	(1.06)
FAMSIZE	...	...	...	...	43.73	45.89	47.30	51.57
	...	...	...	...	(2.73)	(2.85)	(2.75)	(2.95)
CHILDREN	...	...	...	...	11.91	10.60	34.55	29.38
	...	...	...	...	(0.41)	(0.36)	(1.06)	(0.91)
WELFARE	...	...	...	...	–0.04	–0.04	–0.05	–0.08
	...	...	...	...	(0.46)	(0.44)	(0.49)	(0.79)
BOTH PTC	...	...	...	...	–119.54	–113.69	–159.75	–140.58
	...	...	...	...	(2.38)	(2.26)	(2.87)	(2.49)
R^2	0.09	0.08	0.08	0.08	0.41	0.41	0.45	0.46
F	4.71	4.47	3.70	3.55	34.15	34.20	32.48	32.87
n	1,024	1,024	852	852	947	947	762	762

Table 8-6 (concl'd.)

	Equation 8.5 (change in hours worked in 1976 versus the 1974 baseline)				Equation 8.6 (hours worked in 1976)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	402.48	412.46	716.06	749.78	333.99	343.59	667.29	711.40
	(1.78)	(1.79)	(2.72)	(2.81)	(1.42)	(1.41)	(2.49)	(2.61)
PANEL 2	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
PANEL 3	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
PANEL 4	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
DWM	61.27	68.64	201.76	110.66	80.18	80.18	225.47	128.55
	(1.10)	(1.12)	(3.20)	(2.95)	(1.25)	(1.25)	(3.50)	(3.34)
DWF	–25.32	–31.66	–130.68	–99.63	–33.59	–33.55	–139.72	–119.77
	(0.33)	(0.31)	(1.58)	(1.00)	(0.31)	(0.31)	(1.67)	(1.16)
DV	–0.01	–0.01	–0.03	–0.04	–0.02	–0.02	0.04	–0.05
	(0.85)	(0.87)	(2.00)	(2.44)	(0.98)	(0.97)	(2.24)	(2.73)
DCHLD	118.67	119.05	152.99	140.18	129.79	130.20	168.28	153.50
	(2.19)	(2.19)	(2.54)	(2.31)	(2.30)	(2.31)	(2.74)	(2.50)
BASEHOURS	–0.69	–0.69	–0.77	–0.76	0.33	0.33	0.25	0.25
	(14.26)	(14.30)	(14.48)	(14.48)	(6.32)	(6.31)	(4.48)	(4.58)
NIC2	482.25	483.41	433.68	410.71	460.64	461.89	417.70	393.16
	(2.75)	(2.76)	(2.24)	(2.11)	(2.52)	(2.53)	(2.12)	(1.99)
NIC3	460.94	462.27	423.45	431.02	444.63	446.00	422.99	429.65
	(2.77)	(2.78)	(2.32)	(2.35)	(2.57)	(2.58)	(2.28)	(2.31)
NIC4	535.77	535.28	488.58	491.09	528.69	528.10	496.55	497.83
	(3.36)	(3.36)	(2.79)	(2.79)	(3.18)	(3.18)	(2.79)	(2.79)
NIC5	588.19	588.30	508.73	517.70	588.30	588.00	517.50	526.62
	(3.61)	(3.61)	(2.88)	(2.92)	(3.46)	(3.48)	(2.88)	(2.92)
EARNABV	–0.03	–0.03	–0.02	–0.03	–0.03	–0.03	–0.02	–0.03
	(0.97)	(0.97)	(0.72)	(0.87)	(0.98)	(0.97)	(0.72)	(0.88)
FABOVE	399.23	359.63	305.09	367.57	378.67	390.45	331.79	399.94
	(2.11)	(2.18)	(1.69)	(2.05)	(2.21)	(2.28)	(1.82)	(2.20)
BREAK	–0.02	–0.02	–0.02	–0.01	–0.02	–0.02	–0.03	–0.01
	(2.40)	(2.34)	(2.86)	(1.99)	(2.41)	(2.36)	(2.95)	(1.99)
AGE	0.35	0.31	–0.88	0.98	0.00	–0.03	–1.39	0.73
	(0.07)	(0.06)	(0.17)	(0.17)	(0.00)	(0.01)	(0.27)	(0.13)
FAMSIZE	83.56	82.91	86.95	87.75	92.29	91.58	96.96	99.25
	(2.87)	(2.83)	(2.85)	(2.82)	(3.03)	(3.00)	(3.11)	(3.11)
CHILDREN	–22.84	–22.05	–21.62	–21.08	–20.60	–19.67	–20.47	–20.71
	(0.47)	(0.45)	(0.41)	(0.40)	(0.41)	(0.39)	(0.38)	(0.38)
WELFARE	0.15	0.14	0.10	0.04	0.15	0.14	0.09	0.01
	(0.69)	(0.65)	(0.46)	(0.16)	(0.67)	(0.63)	(0.39)	(0.03)
BOTH PTC	–131.01	–131.09	–143.51	–134.02	–134.69	–134.75	–147.55	–135.69
	(1.56)	(1.56)	(1.58)	(1.42)	(1.55)	(1.54)	(1.60)	(1.42)
R^2	0.53	0.53	0.59	0.59	...	...	...	...
F	17.53	17.56	18.04	17.81	...	...	...	...
n	285	285	230	230	285	285	230	230

1 Regression technique is OLS for columns 1 to 3 of each panel. The Tobit model was used for column 4 of each panel. Figures in parentheses are *t*-values. Imputed wages are based on columns 5 to 8 of Table 8-3.

Table 8-7

Estimates of Labour Supply Equations, Based on Mincome Data, Women in Double-Headed Families, 1974-77[1]

	Equation 8.3 (change in hours worked from average of four panels)				Equation 8.4 (change in hours worked from the baseline)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	22.84	−92.49	45.23	117.06	594.17	669.61	623.30	624.12
	(0.30)	(1.18)	(0.51)	(1.20)	(5.17)	(5.83)	(4.46)	(4.39)
PANEL 2	−19.87	83.40	−45.19	−28.98	...	...	...	...
	(0.47)	(1.87)	(0.93)	(0.52)	...	...	...	...
PANEL 3	−18.78	145.65	−55.40	−186.88	−23.36	2.83	−40.20	−32.63
	(0.39)	(2.56)	(1.00)	(2.46)	(0.51)	(0.06)	(0.79)	(0.59)
PANEL 4	−52.72	140.92	−80.34	−252.39	−39.63	3.82	−54.05	−28.67
	(0.96)	(2.17)	(1.28)	(2.80)	(0.77)	(0.07)	(0.95)	(0.43)
DWF	−379.35	−478.04	−364.63	−262.07	−185.12	−210.76	−153.16	−91.39
	(8.40)	(7.80)	(7.30)	(4.66)	(4.32)	(3.73)	(3.17)	(1.72)
DWM	305.88	321.35	323.81	93.97	131.67	123.47	105.68	−16.52
	(7.79)	(7.50)	(7.21)	(3.86)	(3.76)	(3.33)	(2.60)	(0.72)
DV	−0.03	−0.05	−0.03	−0.06	−0.02	−0.03	−0.01	0.01
	(3.18)	(4.02)	(2.73)	(5.00)	(1.55)	(1.88)	(0.82)	(0.33)
DCHLD	−106.36	−102.09	−100.13	−103.66	−148.08	−150.46	−136.56	−141.89
	(4.23)	(4.03)	(3.42)	(3.49)	(5.33)	(5.41)	(4.20)	(4.34)
BASEHOURS	0.00	0.00	0.00	0.00	−0.57	−0.59	−0.55	−0.60
	(0.00)	(0.00)	(0.00)	(0.00)	(17.18)	(17.84)	(15.04)	(15.63)
NIC2	...	...	...	...	−128.96	−120.52	−260.22	−264.41
	...	...	...	...	(1.46)	(1.36)	(2.54)	(2.57)
NIC3	...	...	...	...	−49.63	−45.20	153.53	−140.59
	...	...	...	...	(0.59)	(0.59)	(1.61)	(1.47)
NIC4	...	...	...	...	−86.25	−76.04	−132.38	−112.78
	...	...	...	...	(1.09)	(0.96)	(1.47)	(1.25)
NIC5	...	...	...	...	−53.68	−40.34	−124.04	−112.41
	...	...	...	...	(0.64)	(0.48)	(1.33)	(1.20)
EARNABV	...	...	...	...	0.00	0.00	0.02	0.02
	...	...	...	...	(0.25)	(0.06)	(1.26)	(1.32)
FABOVE	...	...	...	...	−241.86	−223.12	−389.79	−352.00
	...	...	...	...	(2.72)	(2.53)	(3.91)	(3.59)
BREAK	...	...	...	...	−0.01	0.00	−0.01	−0.01
	...	...	...	...	(1.16)	(0.55)	(1.35)	(0.88)
AGE	...	...	...	...	−7.37	−7.94	−6.84	−5.41
	...	...	...	...	(2.79)	(3.02)	(2.21)	(1.57)
FAMSIZE	...	...	...	...	26.87	20.12	30.81	28.79
	...	...	...	...	(1.86)	(1.39)	(1.90)	(1.76)
CHILDREN	...	...	...	...	−153.80	−152.46	−146.86	−141.14
	...	...	...	...	(5.94)	(5.88)	(4.89)	(4.69)
WELFARE	...	...	...	...	−0.03	−0.05	−0.10	−0.17
	...	...	...	...	(0.40)	(0.62)	(1.08)	(1.85)
BOTH PTC	...	...	...	...	327.32	335.84	307.53	319.94
	...	...	...	...	(6.01)	(6.16)	(4.93)	(5.14)
R^2	0.09	0.08	0.09	0.07	0.39	0.39	0.41	0.41
F	5.10	4.59	4.19	3.15	31.46	31.12	27.49	26.86
n	1,096	1,096	884	884	966	966	769	769

Table 8-7 (concl'd.)

	Equation 8.5 (change in hours worked in 1976 versus the 1974 baseline)				Equation 8.6 (hours worked in 1976)			
	W(5)	*W*(6)	*W*(7)	*W*(8)	*W*(5)	*W*(6)	*W*(7)	*W*(8)
Variables								
Intercept	610.21	721.94	636.42	689.94	411.47	608.54	531.73	671.50
	(3.11)	(3.60)	(2.69)	...	(1.17)	(1.69)	(1.27)	(1.61)
PANEL 2	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
PANEL 3	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
PANEL 4	...	...	...	...	...	...	...	...
	...	...	...	...	...	...	...	...
DWF	−183.21	−236.19	−106.42	176.84	−326.91	−375.86	−197.80	−431.29
	(2.73)	(2.60)	(1.39)	(1.78)	(2.84)	(2.41)	(1.49)	(2.30)
DWM	86.77	99.45	31.98	−44.70	153.71	152.97	59.80	−96.08
	(1.46)	(1.50)	(0.47)	(1.16)	(1.50)	(1.33)	(0.51)	(1.39)
DV	0.00	−0.01	0.01	0.01	−0.01	−0.02	0.01	0.04
	(0.02)	(0.35)	(0.28)	(0.27)	(0.14)	(0.40)	(0.26)	(0.72)
DCHLD	−175.35	−176.20	−149.75	−159.65	−269.04	−272.14	−245.04	−255.77
	(3.70)	(3.72)	(2.74)	(2.91)	(3.09)	(3.12)	(2.44)	(2.54)
BASEHOURS	−0.61	−0.63	−0.62	−0.67	0.52	0.49	0.51	0.39
	(11.13)	(11.42)	(10.34)	(10.47)	(5.66)	(5.43)	(5.03)	(3.67)
NIC2	−117.54	−100.86	−219.96	−230.76	−180.98	−159.68	−506.52	−548.71
	(0.76)	(0.65)	(1.26)	(1.33)	(0.63)	(0.56)	(1.57)	(1.70)
NIC3	78.78	85.13	9.32	7.87	82.15	83.49	−73.98	−78.11
	(0.54)	(0.58)	(0.06)	(0.05)	(0.31)	(0.31)	(0.25)	(0.27)
NIC4	1.14	10.44	−69.24	−65.13	40.62	51.86	−136.52	−139.48
	(0.01)	(0.07)	(0.44)	(0.41)	(0.16)	(0.20)	(0.48)	(0.50)
NIC5	64.70	79.30	3.50	−0.78	131.20	150.38	−41.57	−69.27
	(0.45)	(0.55)	(0.02)	(0.00)	(0.50)	(0.57)	(0.15)	(0.24)
EARNABV	−0.02	−0.02	−0.01	−0.01	−0.05	−0.05	−0.03	−0.03
	(0.57)	(0.74)	(0.18)	(0.21)	(1.04)	(1.20)	(0.66)	(0.62)
FABOVE	−154.86	−141.65	−262.24	−265.15	−124.34	−93.06	−219.29	−21,286.00
	(1.05)	(0.97)	(1.57)	(1.65)	(0.52)	(0.39)	(0.80)	(0.81)
BREAK	−0.02	−0.01	−0.01	−0.01	−0.03	−0.02	−0.02	−13.06
	(1.61)	(1.23)	(1.29)	(0.51)	(1.47)	(1.03)	(1.25)	(1.30)
AGE	−11.09	−11.29	−10.89	−6.89	−210.19	−210.19	−20.92	−13.06
	(2.39)	(2.43)	(2.10)	(1.18)	(2.48)	(3.48)	(2.26)	(1.30)
FAMSIZE	33.65	24.98	31.92	35.20	35.42	35.42	52.00	67.39
	(1.32)	(0.98)	(1.14)	(1.25)	(0.77)	(0.77)	(1.05)	(1.34)
CHILDREN	−186.91	−184.82	−169.94	−168.45	−327.43	−327.43	−311.28	−306.83
	(4.41)	(4.36)	(3.56)	(3.54)	(4.19)	(4.19)	(3.52)	(3.48)
WELFARE	0.14	0.12	0.10	−0.02	0.24	0.24	0.20	−0.10
	(0.79)	(0.67)	(0.51)	(0.07)	(0.75)	(0.75)	(0.59)	(0.29)
BOTH PTC	288.25	294.13	290.66	306.17	639.99	639.99	614.91	630.82
	(3.16)	(3.22)	(2.81)	(2.99)	(4.18)	(4.24)	(3.60)	(3.73)
R^2	0.50	0.49	0.53	0.53	...	...	...	...
F	15.39	15.30	14.05	14.17	...	...	...	...
n	285	285	230	230	285	285	230	230

1 Regression technique is OLS for columns 1 to 3 of each panel. The Tobit model was used for column 4 of each panel. Figures in parentheses are *t*-values. Imputed wages are based on columns 5 to 8 of Table 8-3.

assignment, have no effect whatsoever.[3] Thus the concerns about biases arising from assignment and errors-in-variables, which prompt the ad hoc inclusion of these control variables, appear to be alleviated by using equation 8.3. The problem is that the compensated wage effect is negative and significant using this equation, contrary to expectations from economic theory.

As we move to equations 8.4, 8.5, and 8.6 across Tables 8-5 to 8-7, the compensated wage effect becomes positive for married men but not for married or single women. (The result for single men are mixed and generally insignificant because the sample is small. They are included for completeness and will not be discussed.) Income effects are small, generally negative, and often insignificant, as in the baseline analysis in the previous section. Similarly, the cross-wage effects, which are initially positive and significant for both husbands and wives, become insignificant.

One consistent result for double-headed families (Tables 8-6 and 8-7) is worth noting. Changes in the number of preschool children in the family, *DCHLD*, has a significant effect on the labour supply of husbands and wives. Additional preschool children increase the labour supply of the husband and reduce the labour supply of the wife. This may reflect the substitution of market work time for leisure by the husband and non-market work time (raising children) for both leisure and market work time for the wife [Mincer, 1962]. In comparison, the labour supply results are quite disappointing because they are less robust and contrary to theoretical expectations.

Note also that, apart from equation 8.3, pre-experimental labour supply has a large and significant effect. This implies that the bias in measuring virtual income is large according to Keeley et al. [1977, 1978], but their explanation and treatment of this problem is questionable and

Table 8-8

Estimates of Labour Supply Elasticities, Based on the Mincome Experiment, Married Men and Women (Based on Results in Tables 8-6 and 8-7), 1974-77

	Married men				Married women			
	Equation 8.3	Equation 8.4	Equation 8.5	Equation 8.6	Equation 8.3	Equation 8.4	Equation 8.5	Equation 8.6
Compensated wage elasticity								
W(5)	–0.35*	0.05	0.10	0.11	–1.94*	–0.83*	–0.83*	–0.74*
W(6)	–0.32*	0.06	0.10	0.11	–1.77*	–0.62*	–0.68*	–0.54*
W(7)	–0.32*	0.17*	0.32*	0.33*	–1.90*	–0.69*	–0.45	–0.42
W(8)	–0.08	0.15*	0.18*	0.19*	–1.30*	–0.42	–0.75	–0.92*
Income elasticity								
W(5)	–0.07*	–0.06*	–0.04	–0.04	–0.09*	0.05	0.00	–0.01
W(6)	–0.07*	–0.06*	–0.04	–0.04	–0.08*	–0.04	–0.02	–0.02
W(7)	–0.06	–0.10*	–0.10*	–0.10*	–0.09*	–0.03	0.02	0.02
W(8)	–0.04	–0.11*	–0.12*	–0.12*	–0.15*	0.01	0.02	0.06
Uncompensated wage elasticity								
W(5)	–0.42	–0.01	0.06	0.07	–2.03	–0.88	–0.83	–0.75
W(6)	–0.39	0.00	0.06	0.07	–1.85	–0.66	–0.70	–0.56
W(7)	–0.38	0.07	0.22	0.23	–1.99	–0.72	–0.43	–0.40
W(8)	–0.12	0.04	0.06	0.07	–1.45	–0.41	–0.73	–0.86
Compensated cross-wage elasticity								
W(5)	0.33*	0.07	–0.03	–0.10	2.22*	0.73*	0.48	0.43
W(6)	0.29*	0.04	–0.03	–0.03	1.55*	0.63*	0.50	0.39
W(7)	0.30*	–0.01	–0.17	–0.17	2.33*	0.58*	0.17	0.15
W(8)	–0.04	–0.06	–0.13	–0.15	0.68*	–0.09	–0.24	–0.24

***Denotes that elasticity is derived from a statistically significant coefficient estimate.**

Table 8-9

Estimates of Labour Supply Elasticities, Based on the Mincome Experiment, Single Male Heads and Single Female Heads (Based on Results in Tables 8-4 and 8-5), 1974-77

	Single male heads				Single female heads			
	Equation 8.3	Equation 8.4	Equation 8.5	Equation 8.6	Equation 8.3	Equation 8.4	Equation 8.5	Equation 8.6
Compensated wage elasticity								
W(5)	–0.28	–0.05	0.00	–0.07	–0.46*	–0.14*	–0.22*	–0.37*
W(6)	–0.19	–0.01	0.01	–0.06	–0.23*	–0.13*	–0.16	–0.18*
W(7)	0.01	0.12	–0.02	–0.23	–0.20*	–0.07	–0.12	–0.16
W(8)	–0.01	0.10	–0.05	–0.37	0.76*	0.18	–0.20	–0.10
Income elasticity								
W(5)	0.19	0.21	0.11	0.14	–0.10	0.07	0.22	0.23*
W(6)	0.12	0.17	0.06	–0.09	–0.16*	0.00	0.11	–0.02
W(7)	0.24	0.40*	0.62*	0.66*	–0.10	0.00	0.11	0.04
W(8)	0.23	0.40	0.61*	0.61*	–0.28*	–0.05	0.08	0.03
Uncompensated wage elasticity								
W(5)	–0.09	0.16	0.11	0.07	–0.56	–0.07	0.00	–0.14
W(6)	–0.07	0.16	0.07	–0.15	–0.39	–0.13	–0.05	–0.20
W(7)	0.25	0.52	0.60	0.43	–0.30	–0.07	–0.01	–0.12
W(8)	0.22	0.50	0.56	0.24	0.48	0.13	–0.12	–0.07

*Denotes that elasticity is derived from a statistically significant coefficient estimate.

inspires little confidence in the results. The fact that the results for the wage effects for husbands change dramatically as we move from equation 8.3 to equations 8.4, 8.5, and 8.6 adds to our concerns.

The results are summarized in Tables 8-8 and 8-9, which compute the labour supply elasticity for each equation estimated. The elasticity estimates for husbands are small as most previous literature suggests. The compensated wage elasticity ranges from –0.35 for equation 8.3 to 0.33 for equation 8.6. The income elasticity is more uniform, ranging from –0.04 to –0.12. The compensated cross-wage elasticity is generally insignificant and ranges from –0.17 to 0.33.

The estimates of the compensated wage elasticity for wives are negative, larger, and generally significant in each case, varying from –1.94 to –0.42. The income elasticity is small and generally insignificant, ranging from –0.15 to 0.06. The compensated cross-wage elasticity is generally positive, ranging from –0.24 to 2.33. For single female heads, the compensated wage elasticity is also negative and quite large for the most part, ranging from –0.46 to 0.76. The income elasticity for single female heads ranges from –0.28 to 0.23 but is mostly insignificant.

Conclusion

The results from the Mincome experiment are, in one sense, disappointing. The estimates of the compensated wage elasticity are often negative and statistically significant, in defiance of economic theory. Moreover, different model specifications (equations 8.3 to 8.6 for the four wage equations) yield rather different estimates.

In another sense, however, we see the estimates as quite valuable. For various model specifications the elasticity results are consistently small in contrast to estimates from the Mincome baseline data. The compensated wage elasticity never exceeds 0.33 for husbands, 0 for wives, and 0.76 for single female heads, while the income elasticity is never below –0.12 for husbands, –0.15 for wives, and –0.28 for single female heads. If we ignore perverse results on theoretical grounds (negative compensated wage elasticities and positive income elasticities), these results imply a range of uncompensated wage elasticities of –0.12 to 0.33 for husbands, –0.15 to 0 for wives, and –0.28 to 0.76 for single female heads. The results confirm the small elasticity estimates obtained by Mroz for married females in contrast to the much larger estimates obtained in some other studies.

They also confirm the small estimates for husbands, primarily obtained from earljer literature.

The Mincome results also show that the estimates by Keeley et al. [1978] from the Seattle-Denver experiment lead to larger compensated wage elasticities and smaller income elasticities, at least for husbands in the Mincome data. In view of this result and the other questions raised about their estimator, we advise caution in the use of their results for policy analysis.

One novel feature of our analysis is the focus on family labour supply and the compensated cross-wage elasticity. This elasticity is generally positive for wives and, in some models, is large and statistically significant. The elasticity is smaller and less often significant for husbands. The results do not suggest that we can easily ignore the cross-wage effect in double-headed families, at least for wives.

An important additional factor, also a novel feature of our analysis, is the effect of children on labour supply. Preschool children increase labour supply for the husband and reduce it for the wife. These results are significant and consistent across all model specifications. Indeed, this is likely the strongest effect in the Mincome data and it deserves further investigation.

Part C

Conclusions

9 Lessons for Income Maintenance and Social Policy

Poverty in Canada is an enduring social issue. Public interest in income maintenance reform peaked in the 1960s, leading to extensive research and five social experiments designed to assess the effects of a guaranteed annual income or a negative income tax. Of special importance was the question of labour supply, or work incentives, under such a program. But despite a clear theoretical understanding of the problem, wide availability of numerous data sets of high quality, and impressive econometric analyses of the question, the effects of alternative income maintenance programs on labour supply remain somewhat controversial. Our study addresses this controversy in two ways – first, by a review and assessment of labour supply research to date in the United States and Canada (Part A) and, second, by the first analysis of labour supply behaviour in the Canadian experiment, Mincome (Part B). The contributions of the study include an update of earlier surveys of U.S. research, a survey of Canadian research on the issue, specific emphasis on family labour supply, and an assessment of the Canadian experimental evidence in the context of earlier experimental and non-experimental research.

What can we conclude from this vast array of research? Is there an emerging consensus on labour supply research and its implications for income maintenance policy? What are the implications of this research for other policy initiatives? In this concluding chapter, we assess these questions in two sections. First, we summarize and assess the labour supply evidence from the study. Second, we consider the policy implications of our findings.

The Labour Supply Evidence – Much Ado about Nothing?

Labour supply research provides an acid test for empirical research in economics. The theoretical foundations, particularly the static labour supply model set out in Chapter 2, are sound and well understood. The data that have emerged over the last 25 years, particularly in the United States, are better than that available for other research issues [Stafford, 1986]. And the policy questions, particularly the one concerning the labour supply response to a negative income tax, have been clearly articulated. Furthermore, public support and government commitment to a reliable answer to the question appeared exceptionally strong. If economic analysis cannot deliver policy directions in this area, then we would have to wonder about the relevance of economic research in today's world.

Our conclusion is guarded but reassuring. Our research leads us to conclude that labour supply research has resolved some issues and has provided some consensus on the likely range of behaviour. Much of this consensus has been achieved only in recent years and after 20 years of intensive research. However, there remain many loose ends. Hence, good applied research – blending economic theory with appropriate data and statistical analysis – is still needed in studying labour supply.

The early literature reviewed in Chapter 2 concentrated on non-experimental cross-sectional data to estimate labour supply response [Cain and Watts, 1973]. The results suggested generally small compensated wage and income elasticities for men, although DaVanzo et al. [1976] demonstrated the sensitivity of the results to the treatment of non-workers. The estimates of the compensated wage elasticity were found to be large and varied for women. This finding, when combined with small income elasticity estimates similar to those for men, implied larger uncompensated wage elasticities for women. This led to the conventional wisdom that male labour supply response to taxes and income transfers was weak or inelastic, while female labour supply response (particularly that of married women) was stronger and deserving of more attention. Cross-wage elasticities linking labour supply behaviour to the budget constraint of the spouse were ignored or poorly estimated.

Subsequent research, reviewed in Chapter 3, has taken two directions. First, there were a number of econometric refinements to the analysis of cross-sectional data. In particular, the refinements addressed the treatment of non-workers and the treatment of non-linear budget constraints arising from the current tax-transfer system. While these were path-breaking developments in applied econometric research, they did little to resolve the large variation in estimates for female labour supply response. Indeed some of the prominent estimates of the compensated wage elasticity for females were very large [Heckman, 1976 and 1980; Cogan, 1980 and 1981] while others, particularly many of those using Canadian data, were quite small [Nakamura et al., 1979; Nakamura and Nakamura, 1981; Stelcner and Smith, 1985]. None of the studies dealt with both these issues satisfactorily and none resolved the empirical issues. Second, there were also a number of data refinements, particularly the development of experimental panel data. These data permitted researchers to control for omitted fixed effects, such as individual motivation, by comparing the behaviour of the same individual over time. They also allowed researchers to obtain a better measure of labour supply and to avoid such complex issues as non-linear budget constraints and constrained labour supply. The results from four U.S. experiments are somewhat difficult to interpret because of differences in experimental treatment and only infrequent estimation of structural labour supply models. Nevertheless, the results suggested almost uniformly small elasticity estimates for both males and females [Robins, 1985].

An important study by Mroz [1987] reconciles much of the labour supply evidence. His study carefully tests various specifications of the labour supply model, various treatments of non-workers, and the effect of non-linear budget constraints for married women in the United States. He concludes that previous studies using cross-sectional data are fraught with specification errors, particularly the use of work experience as an instrument for the wage rate. This misspecification appears to account for the large estimates of the compensated wage elasticity in earlier studies. Mroz's results indicate smaller elasticity estimates within the range of earlier results for males. Since this misspecification is not a factor in the analysis of experimental data where wage variation is large by design and where individuals are compared to themselves at an earlier time rather than to other individuals, Mroz's results also suggest why experimental estimates by labour supply response have been smaller and more uniform.

In Chapters 7 and 8, we re-examine this argument with an analysis of the experimental data from Mincome. In Chapter 7, we focus on the experimental effect per se using ANOVA and ANCOVA models of the four-year panel. Many of the models estimated take account of specific problems associated with experimental and/or panel data: attrition, non-random assignment, and the effect of break-even status. The results indicate very small labour supply response for all groups – a reduction in hours worked of about 1 per cent for men, 3 per cent for married women, and 4 per cent for single women. These results are also small in comparison to the U.S. results, although not far from the comparable estimates for the New Jersey and Gary experiments. The Seattle-Denver results are considerably larger, although the implied labour supply elasticities are still small in comparison with evidence from cross-sectional data.

Chapter 8 presents estimates of structural labour supply models and, hence, labour supply elasticities that permit comparison with non-experimental research and facilitate general evaluation of income maintenance and other social policies. Two types of estimates are presented. First, we estimate the labour supply models in various conventional ways using the cross-sectional baseline (pre-experimental) data. The estimates of the labour supply elasticities for both males and females are varied and often very large, confirming our reservations about cross-sectional evidence without careful specification-testing. Second, the labour supply model is estimated using the panel data. We follow the methodology developed by Keeley et al. [1978] up to a point, but we criticize their estimation process and opt for a more suitable within-groups estimator of the panel data. The estimates of the labour supply elasticity, as in studies from earlier U.S. experimental evidence, are small and sometimes disagree with theoretical predictions that the compensated wage effect should be positive and the income effect should be negative. The results are also smaller for the within-groups estimator than for the Tobit model of Keeley et al. Our estimates imply the following range for men, married women, and single women.

Table 9-1

A Summary of Labour Supply Elasticity Estimates from Mincome

	Elasticity estimates		
	Compensated wage	Income	Uncompensated wage
Men	0 to 0.3	0 to –0.1	–0.1 to 0.3
Married women	0	0 to –0.2	–0.2 to 0
Single women	0 to 0.8	0 to –0.2	–0.2 to 0.8

SOURCE Tables 8-8 and 8-9.

These estimates are small in comparison with cross-sectional evidence but consistent with other experimental results and with Mroz's results for married women. Our analysis also provides unique estimates of the compensated cross-wage elasticity for double-headed families. These estimates are small for husbands and generally insignificant, but large, positive, and statistically significant for wives. Although cross-wage effects have been ignored in most of the empirical literature, our results should encourage more research on this issue, especially with the trend to two-worker households and increasing recognition that labour supply is a family-based decision. Models and policy simulations that ignore cross-wage effects may produce seriously biased results.

In summary, our research leads us to the conclusion that the labour supply response to changes in the tax-transfer system, such as those involved in income maintenance programs, will be small, with the possible exception of cross-wage effects for married women, which require much more research. Although precise measurement of labour supply response is a very difficult problem, and one that economists and econometricians have not yet mastered, we have narrowed the range of reasonable estimates to those that indicate that individuals and families are likely to be fairly insensitive to changes in the tax-transfer system facing them.

Policy Experiments – Yesterday's Failure or Tomorrow's Hope?

Labour supply response is an important issue in income maintenance and other social policy evaluation since work disincentives are a powerful argument against such program initiatives. Time and again, suggestions to reform either income support or social insurance programs in Canada have pressed against the fear that altering benefits for those capable of work might prove counterproductive. At its core, the worry is that cash transfers would diminish work incentives.

Our results suggest that these fears are largely misplaced, since the labour supply response to changes in the tax-transfer system are estimated to be small. Thus the work disincentive effects of alternative income maintenance programs are unlikely to be a deciding factor. For example, one suggested alternative to the negative income tax/guaranteed annual income scheme has been wage subsidization because of the less serious work disincentive effects [Kesselman, 1969]. Yet the difference between the two programs revolves around the size of the compensated wage elasticity, which we now believe to be very small. Since the guaranteed annual income scheme is much better at delivering income supplementation to all those in need, not only low-wage workers [Hum and Simpson, 1986], the case for a guaranteed annual income to eliminate poverty is strengthened by our findings.[1]

Implications for other social programs may also be drawn, albeit tentatively. The estimates of the structural labour supply elasticity may be used to assess the labour supply implications of other programs as well. Income supplementation programs – such as current social assistance programs, unemployment insurance, and pensions – should have only modest work disincentive effects because the income elasticity of labour supply response is estimated to be small across all groups. Some care must be taken not to overstate this conclusion, however, because the total response depends upon the size of the individual response as well as the income elasticity for the population. Thus very small effects over a large population group may prove substantial and should be properly evaluated. Furthermore, the manner in which a particular program is administered may have substantial influence, and these elements are not captured by estimates of the structural labour supply. Still, when evaluating the probable effects of work response, estimates of the structural labour supply are preferable to guesses derived from specific program experiences.

Equally important from an economic standpoint are the implications for the deadweight or welfare cost of the taxation of earnings. While some studies in the United States have argued that the cost of such taxation is high [for example, see Browning and Johnson, 1984], we would argue that much of this position rests on inflated estimates of labour supply response (the compensated wage elasticity). Thus we would conclude that the cost of income taxation to society, such as the cost of income supplementation and redistribution, has often been overstated based on outdated evidence about labour supply response. Much of this debate has taken place in the context of U.S. evidence and programs, but interest is also strong in Canada. For example, support for our argument is provided by Cloutier and Fortin [1989] who estimate the welfare cost of taxation using a computable general equilibrium model of the Canadian economy. They use fairly large "benchmark" elasticity estimates to show that a marginal increase in personal taxes would produce a marginal welfare cost of 156 per cent of the additional tax (or transfer reduction). But halving their elasticity estimate would move it into the range found acceptable in this study and reduce the marginal welfare cost to 65 per cent of the additional tax. As the wage elasticity approaches zero, so do the welfare costs of change in the tax-transfer system. The point to be noted is clearly the central importance of reliable estimates of labour supply elasticities when evaluating social policy changes, whatever the detailed proposal under discussion and whatever the model framework.

Our review of the evidence on labour supply behaviour has convinced us that experimental data have proven a valuable source of information for evaluating income maintenance policy. We reject the view that the experiments in the United States and Canada were not worthwhile. In our view, the experiments generated much more uniform and precise estimates than the non-experimental literature, gave impetus to many analytic advances in labour supply research, and provided administrative guidance for future policy development. We believe that many quarters have seriously undervalued these benefits.

Indeed, it is our view that policy experimentation should be considered for other social policy issues, such as employment and training. The experience gained from previous social experimentation, especially in the United States, provides a basis for effective experimentation in other areas of Canada. Many of these areas, as with labour supply and income maintenance, are littered with analytical complexities that defy confident evaluation with non-experimental data and could benefit decisively from well-conceived social experimentation. While the cost of social experimentation is high, so too are the costs of repeated non-experimental research and ill-advised social policy decisions.

The North American social experiments in income maintenance were an audacious and remarkable achievement, and Canada should be justly proud that Mincome was part of this innovative research endeavour. This achievement is secure, notwithstanding the tardy appearance of results respecting labour supply. Much more analyses of the Mincome data are possible. And it bears repeating that the Mincome data set remains the most richly detailed longitudinal data on labour supply behaviour in Canada today. In time, its lessons will yield to further research and inquiry. For now, we may consider the special nature of Mincome's place as a pioneering symbol.

The income maintenance experiments reflected both their time and place. The policy debates on reforming welfare and aiding the poor had crystallized around one central question – that of work disincentives – and there seemed little purpose to further discussion without "getting the facts." But getting the facts was no easy matter, and when the facts appeared, they were not so simple. Social experimentation, with its promise of decisive and definitive testing, proved unable to quell the doubts, compel consensus, or inspire government to enact programs to combat poverty. In fact, economic analysis is seldom the decisive factor in determining public policy, so we should not judge the success or worth of social experiments such as Mincome with reference to enacted legislation.

The lessons from Mincome for economic researchers is another matter. Despite its many advantages, social experimentation brings with it novel technical twists perhaps no easier to deal with than the difficulties that explicit experimentation was supposed to eliminate. Furthermore, economic researchers interested in participating in discussions of public policy are forewarned to have stable jobs, secure tenure, and plenty of time and leeway to pursue questions for the proverbial long run. Social experiments such as Mincome possess too long a gestation period for their research to make a difference to short-term policy discussions. Their value must therefore be measured in terms of their ability to address questions of public policy that are enduring in nature, and such that reform proposals that emerge from time to time can be recast into terms amenable to answer by the experimental data, whether this recasting be obvious to policymakers or not. Thus Mincome makes a contribution to the policy debates respecting income maintenance because of its potential to suggest the likely effects of work response, whether to a guaranteed income or some other measure. The capacity to estimate structural labour supply functions for individuals and families ensures this role for the Mincome data set, regardless of the details of specific policy suggestions.

In short, the contribution of Mincome to social policy lies less in its failure to introduce a guaranteed income for Canadians shortly after the experiment's end; rather it is to be tallied in terms of its legacy of data and research opportunities. Still, the experiment represents a powerful rallying point for those who want their arguments couched in the most direct of terms. To those who still insist that the work disincentive effects of a proposed guaranteed annual income or a negative income tax are sheer academic speculation, we can now say that Canada actually tried a guaranteed annual income program, that the facts are in, and, while sincere disagreement exists as to the precise degree of effect, we are not likely to do better, nor will Canada likely undertake another social experiment on the scale of Mincome again.

Notes

CHAPTER 1

1 The proposals by Friedman, Lampman, and Tobin during this period were all based upon considerations of fiscal efficiency and tax equity. Theobald [1963] argued for a guaranteed income based upon the view that new technology and automation would increasingly displace humans in the work process. He therefore advocated a guaranteed income as an absolute and constitutional right, not because it had anything to do with personal income tax mechanics. In a similar vein, Schwartz [1964] also advocated a guaranteed income based upon his social work philosophy. The income allowance approach taken by Tobin can be traced back to Lady Rhys-Williams [1942, 1953] who proposed weekly cash dividends together with a work test. Among Canadians, Baetz [1970] advocated a guaranteed income on the argument that such a right was inherent in the United Nations Universal Declaration of Human Rights, which Canada had endorsed. An early proposal for unconditional income allowances was suggested by Smith [1965], a businessman and chartered accountant. During the early 1960s, Canadians appeared less preoccupied with poverty levels than to quibble over Porter's [1965] portrayal of middle-class aspirations in *The Vertical Mosaic*.

2 The proposition seemed so simple. If you want information about some program that does not exist, try it out! The credit for the initial idea goes to Heather Ross, a graduate student in economics at the Massachusetts Institute of Technology working with the Council of Economic Advisers during the summer of 1965. Although her specific proposal was not accepted, it received wide circulation and many econometricians strongly endorsed the idea of an experiment [Orcutt and Orcutt, 1968].

CHAPTER 2

1 The typical textbook case is that of a single worker-consumer. However, in order to discuss family labour supply and how changes in the wages of one person affect the work behaviour of the spouse (the cross-wage effects), we need at least two workers. Hence we start with two adult workers at the outset.

2 For a review of the issues surrounding the family utility function, see Killingsworth and Heckman [1986], pp. 126-34.

3 Keeley [1981, Chapter 3] illustrates this point quite well. He decomposes the labour supply effects of income maintenance programs, wage subsidy programs, income tax changes, and public employment programs into compensated wage and income effects.

4 For further discussion of the problems involved in interpreting labour supply estimates from aggregate data, see Pencavel [1986], pp. 31-35.

5 That is, wealth accumulation is itself a stochastic variable related to preferences or ambition as well as past labour supply. For example, if the unobservable character traits of "industriousness" or "laziness" influence hours of work, *ceteris paribus*, then those individuals who are more industrious will, after a time, have more wealth, given the same family situations, savings propensities, age, and so on. Wealth is therefore correlated with the error term in the labour supply function, creating estimation bias.

6 For example, determination of wage rates from dividing earnings by hours (the dependent variable) results in bias [Borjas, 1980]. This bias can be circumvented by using an imputed wage whose determinants are uncorrelated with the error term in the labour supply equation. In practice, however, this condition is difficult to ensure.

CHAPTER 3

1 1976 is the middle year of the Mincome experiment to be analysed in Part B of this study. The features of the tax-transfer system in Manitoba that produce non-linear and non-convex budget constraints are common to other Canadian provinces [Stelcner and Smith, 1985] and the United States [Hausman, 1980].

2 Although MaCurdy uses experimental data, he only uses the control group. His data are therefore equivalent to other non-experimental data sources cited in Table 3-1.

3 Prescott et al. [1986] provide sufficient information (mean wages, hours, and income) to calculate the labour supply elasticities from their regression results.

4 Another solution is to wait for reforms to the tax-transfer system to compare behaviour before and after the reforms using non-experimental panel data. Such studies are likely in progress, since there has been tax reform recently in both Canada and the United States. Canadian research will be hampered by the lack of a continuing longitudinal survey, however. The Labour Market Activity Survey panels for 1986 and 1987 are not being continued, although a new panel has started.

5 Other control variables include age and family status. If these independent variables were uncorrelated with the experimental treatment variables, as would be expected in a non-random assignment of plans, then their inclusion would not affect the labour supply estimates.

6 We will return to direct comparison of plan effects in Part B where we estimate analysis-of-variance models for Mincome.

7 Prospects for further research on this question are limited. The Seattle-Denver experiment enrolled 169 experimental and control families in a 20-year program in 1974, but the program was terminated in 1980. Robins [1984] finds little evidence that those families behaved differently from families enrolled in three-year and five-year programs, but the small size of the 20-year sample provides little confidence in the results.

8 Specific solutions are provided by Killingsworth [1983, pp. 236-39], Altonji [1986], and Jakubson [1988].

9 Estimates for males refer to the seven studies in Killingsworth [1983, Table 5-1, pp. 298-300], that is, Becker, Smith, Lillard, Ashenfelter and Ham, MaCurdy (two studies), and Altonji, plus Altonji [1986]. Estimates for females include four studies in Killingsworth [pp. 300-1], that is, Smith, Heckman and MaCurdy (two studies), and Altonji, plus Jakubson [1988]. Results for aggregate time series are ignored, although estimates are in the same range.

Chapter 5

1 The Canada Assistance Plan Act remains the most significant umbrella program for cost-sharing social assistance. For a detailed discussion of the plan, see Hum [1983].

2 For a discussion of family allowances, Quebec's position, and federal-provincial relations during this period, see J. Saywell, ed., *Canadian Annual Review of Politics and Public Affairs, 1972* (Toronto: University of Toronto Press, 1974), pp. 88 ff.

3 An appraisal of the welfare system of Manitoba by professor Clarence Barber was published on December 22, 1972. His report recommended the introduction of a guaranteed income. On the day the report was released, Premier Schreyer announced that Manitoba was "on the brink" of concluding a federal-provincial pilot project. It is difficult to tell how much influence Barber's report had.

4 Cf. the following definition given by Riecken and Boruch [1974], p. 3: "by experiment is meant that one or more treatments (programs) are administered to some set of persons (or other units) drawn at random from a specified population; and that observations (or measurements) are made to learn how (or how much) some relevant aspect of their behaviour following treatment differs from like behaviour on the part of an untreated or control group also drawn at random from the same population."

5 No derogatory intent is implied by the distinction between experiments on the one hand and demonstrations and pilot projects on the other. The aim is simply to clarify the term experiment since many policymakers are accustomed to thinking loosely of any trial program as "experimental." It is not clear what Schreyer had in mind.

6 A more detailed summary of the design of Mincome is given in Chapter 6. The complete design and sample details of the program are contained in two technical documents by Hum, Laub, and Powell [1979] and Hum, Laub, Metcalf, and Sabourin [1979].

7 The Institute for Social and Economic Research folded in 1986. Its successor, University of Manitoba Research, Social Sciences Division, stopped operations in 1988.

Chapter 7

1 We exclude the Dauphin and rural samples and families that split during the experiment. The Dauphin sample is unique among all the income maintenance experiments in that it involves a single plan that was made available to all Dauphin residents. The sample deserves special attention and would require additional data preparation. The rural sample suffers from many of the same problems as the Rural Income Maintenance Experiment in the United States and appears to be a much less promising source of information on labour supply response than the Winnipeg or Dauphin samples.

2 We present evidence of the substantial variation in after-tax wage rates and income guarantee levels in Mincome in Chapter 8 when we consider structural labour supply models.

3 For further discussion of the analysis of the break-even status in the U.S. experiments, see Keeley [1981], pp. 131-43.

4 Here there is a control group for all time periods and all those in the experimental group are part of the control group in the first pre-experimental year.

5 The sixth, and highest, normal income cell was combined with the fifth income cell during the experiment.

6 Participants who quit after the pre-experimental survey are ignored here because we cannot compute fixed-effect models without at least two observations per household.

Chapter 8

1 Errors in the measurement of hours worked induce a spurious negative correlation between labour supply and the wage rate when wages are determined by the ratio of earnings to hours worked. A positive error, for example, increases labour supply and reduces the ratio of earnings to hours.

2 Supplementary Tables 1 and 2 are published separately because of their size and complexity. They are available from the Economic Council of Canada upon request.

3 This turns out to be a necessary consequence of adding baseline control variables to the within-groups estimator. We may write equation 8.3, ignoring time effects for simplicity, as $Y_{it} - Y_i \equiv Q_v Y_{it} = Q_v Z_{it} \beta + Q_v W_i \delta + Q_v e_{it}$, where Q_v is defined in Hausman and Taylor [1981, p. 1380], and $X_{it} = (Z_{it} \mid W_i)$, where W_i is a set of baseline control variables for individual i. Then OLS estimation yields $\hat{\delta} = 0$ because $Q_v W_i = 0$, when W_i only includes baseline control variables such as hours worked. Simply put, W_i acts like a vector of fixed effects.

CHAPTER 9

1 In fact, Hum and Simpson [1986] conclude that the work disincentive effects are small over a broad range of assumptions about labour supply elasticities because of the extremely high tax (benefit reduction) rates currently facing the poor. On the other hand, the low correlation between low-wage earners and low-income families makes wage subsidization an inefficient method to address family income maintenance.

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